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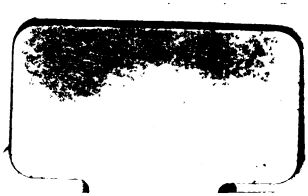

BRAITHWAITE'S RETROSPECT.

VOL. LXXVIII. JULY-DECEMBER, 1879.

THE

RETROSPECT OF MEDICINE:

BEING

A HALF-YEARLY JOURNAL

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

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LATE LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN AT THE LEEDS SCHOOL OF MEDICINE, ETC.

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SYNOPSIS,

(ARRANGED ALPHABETICALLY), CONTAINING

A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THIS VOLUME, SHOWING AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE HALF-YEAR.

AFFECTIONS OF THE SYSTEM GENERALLY.

ACUTE RHEUMATISM.—It is at present impossible to distinguish those cases who are likely to take the medicine with rapidly good effect, and without any unpleasant results, from those who are intolerant of it. But it may be stated that persons in great pain, and with high fever, and in whom there is not, when the treatment is commenced, any complication, are, as a rule, the most favourable cases for it. Still slight complications, whether cardiac or pulmonary, should not preclude the treatment by salicylate of soda. Indeed, cases occur in which the drug produces rapid relief of the pain and joint affection, and no unpleasant symptoms whatever, notwithstanding the presence of pretty serious complications. Usually, however, the drug seems to have very little effect in modifying the course of cardiac or pulmonary affections occurring in acute rheumatism, although it may reduce the temperature in spite of them. Any one who has seen many cases of acute rheumatism treated by salicylate of soda must, I think, allow that its discovery as a cure for that disease is a triumph of empirical therapeutics, which has probably had but few parallels in the history of medicine. (Dr. S. J. Sharkey, p. 63.)

CHOLERA.—*Boracic Acid.*—Mr. Butler, Surgeon Madras Medical Service, has necessarily had a good deal of experience in the treatment of cholera. The most successful plan of treatment only yielded however a percentage of 45 to 50 recoveries. He says, I was induced to consider whether any more efficacious remedy could not be resorted to. About this period the properties of boracic acid had recently been made public, and determined to try its effects. The pure acid not being readily procurable, the biborate of soda was at first employed with marked benefit, the percentage of recoveries rising from 70 to 75 per cent. Subsequently the pure acid was employed in

ten-grain doses every two hours, combined with sodæ bibor., or sodæ bicarb., under which treatment every case recovered. (Mr. W. J. Butler, Lahcet, Aug. 24, p. 257.)

CONTAGION AND INFECTION.—*Destruction of.*—What power have we of destroying contagion within the body? This is a question beset with many difficulties. It is quite clear that if we are to rely on disinfectants, these must be introduced into the system in sufficient quantity to cause the death of the living particles of contagion; and in doing this we must bear in mind that we do not cause the death of our patient by poisoning him with our remedies, or that we do not seriously damage or destroy some of the delicate organisms which are necessary to his healthy existence. Fortunately we are possessed of disinfectants whose powers are not easily impaired by the presence of organic matter; these are quinine, salicylic acid, and *possibly* also thymol, carvol, eucalyptol, and other recently investigated antiseptics and disinfectants. The power of these agents to destroy contagion has not yet been properly tested, but estimating their disinfectant by their known antiseptic powers, we have good reason to conclude that they are disinfectants. Quinine and salicylic acid possess this advantage, that they can be introduced into the system in large quantities without injury to the patient. We have, therefore, two drugs which have the power of acting as disinfectants within the system. This power, indeed, seems to have been to a certain extent demonstrated in the case of these drugs. We know that the effect of the action of any contagion within the system is to produce fever, and that the essential symptom of fever is a high temperature. We know that quinine and salicylic acid, administered in large doses, reduce temperature—in fact, suspend for a time the febrile processes, and the continued use of these drugs reduces fever. (Dr. T. W. Grimshaw, p. 13.)

FEVER.—*Cause of the preternatural Heat of Fever.*—The greater part of the preternatural heat of fever is due to increased formation of heat by increased combustion of the tissues, especially the nitrogenous tissues. During fever the quantity of urea is largely increased, and this is generally considered to depend on general combustion of the nitrogenous tissues. Some have demurred to this conclusion, maintaining that the increase of urea is due to the retrograde metamorphosis of morbid products formed by the disease, as the exudation into the lungs of pneumonia. This explanation, however, fails altogether to explain the increase of urea in an attack of ague, where no morbid products are formed. Some years ago I showed that during a paroxysm of ague the urea is increased

in proportion to the height and duration of the fever, so that given the height of the fever we can approximately calculate the increase in the urea; and, *vice versâ*, given the increase of urea, we can ascertain the height of the fever. The increase of the urea begins directly the fever begins, and declines with its decline. This increase in the urea must be ascribed to increased combustion of nitrogenous tissue; and as the increase of urea is in proportion to the severity of the fever, it is fair to conclude that this increased combustion causes the fever. (Dr. Sydney Ringer, p. 53.)

Alcohol in Fever.—The therapeutic effects of alcohol, when exhibited in certain typical forms of fever, are most marked. The cases which require it are those in which there is a dry tongue and skin, no sickness and no indication of cerebro-spinal lesion. If there is any indication of the latter with a moist tongue, stimulants universally do more harm than good, because the tendency is then to increase the congestion upon which the lesion depends. When a case is benefited by alcohol, there is soon produced a lessened temperature, a slower pulse, a moister tongue, and a quieter condition of system generally. The beneficent effect of alcohol in such cases supports the theory which I hold that such fevers are the manifestation of an action which is taking place in the blood of a person who is not in a proper state of health. Some foreign material is retained, or some matter, which ought to have been excreted, exists there in excess of its proper quantity, a specific zymotic germ or some molecular matter, in a state of change, finds entrance into the circulation, and, finding a fitting pabulum, the specific substance is reproduced in quantity, being matured out of the impure material which ought not to be there, supplemented by some other tissues of the body. The growth of this produces more heat than is ordinarily developed, rising at times higher than that which is compatible with life. It is produced at the expense of retained excreta combined with some part of the body which is necessary to support life. Alcohol in such cases comes to the rescue, interfering with the rapid tissue change, and is itself consumed, whereby temperature is lowered, and the morbid matter is not produced so rapidly as would have been the case if the alcohol had not been exhibited; oxidization goes on at a lower temperature, there is a decrease of the production of carbonic acid, and less hurtful products result. In these cases the effect of alcohol has to be most carefully watched, and the moment there is evidence of cerebro-spinal lesion or of alcoholic excess its use must be discontinued. I have seen patients in serious danger, which

has been brought about by the wine and spirits which have been too freely exhibited, rather than by the disease itself. (Dr. A. Carpenter, Med. Press and Cir., May 29, p. 440.)

The Graduated Bath in Fever.—Temperature may be lowered by the bath when the salicylate fails. In one case, the salicylate of soda was given from the first in doses of thirty grains every two hours, and was stopped at the seventeenth dose. No nervous symptoms are recorded; but sudamina were abundant and resembled croton oil rash, while the urine was albuminous. Recourse was had to the bath because the temperature had risen briskly during the administration of the salicylate, and after the seventeenth dose had reached the dangerous figure of 106·4°. The bath was graduated from 90° to 64° during half-an-hour, and at the end the patient's temperature was 96·6°, a fall of nearly 10° Fahr. The fever was completely subdued, normal temperature being permanently resumed on the fourth day after the bath; the patient was allowed to get up on the twelfth day, and was presented on the fifteenth day. The fact of the complete failure of the salicylate though given in full and rapidly repeated doses is the first point of interest in these cases. If the doses be counted up, 360 grains of salicylate will be found to have been given in forty-eight hours in the first case, and 510 grains in thirty-six hours in the second. The toxic influence of the drug was well declared, by noises in the head and giddiness in the one, by albuminuria in the other. Nevertheless the temperature continued to rise in both instances to 106° or upwards. The effect of the bath was in both to produce an immediate fall of between 9° and 10° Fahr., and a complete cessation of pain, restlessness, and other symptoms attendant on fever. And the effect was also lasting, the rebound not reaching in either case within 3° of the acme, and being followed by rapid defervescence. In typhoid, as in acute rheumatism, the bath is the more energetic remedy of the two, and the safer. No evil results of any kind can be traced to its use, the only objections to it resting on grounds of trouble and exposure, such as ordinary care and nursing power ought to exclude. The cases presented in abstract appear to me so satisfactory as to justify, or perhaps even to demand, the use of the bath in all cases of hyperpyrexia in enteric fever where the moving of the patient is not contra-indicated by peritoneal complication or perforation. Even then the cool pack or cold sponging are valuable substitutes. The rule at present adopted with reference to the bath is that it should be used, with the above necessary exceptions, whenever in enteric fever the temperature having reached 105° is still

rising, particularly if the rise be before or after the usual evening hour. As I have claimed before, the practice, though not uniformly successful, has not in any instance done any known harm, and has, in all probability, averted death in several instances. Accordingly, notwithstanding some difficulties arising in the carrying out of the process of bathing, I propose to adhere to this rule in all cases of enteric fever coming under my care. (Dr. W. M. Ord, p. 32.)

MALARIA.—*The Action of.*—When malaria exists in a latent or masked form in the human body, it will excite and complicate any disease to which the body, from some peculiar temperament—be it nervous or sanguineous—may be disposed. Thus, if the constitution be disposed to asthma or to hysteria, the patient under the influence of malaria may be seized with a pernicious asthmatic or a pernicious hysterical fever. Should he be disposed to rheumatism or to liver disease, there may be a complication of pernicious icteric or pernicious endocarditis. The form of malarial disease will always be characterised by some prominent symptom of the special diathesis. The spleen is the organ which suffers most frequently from the influence of the poison of malaria; a reservoir of nutritive blood materials, to be drawn upon or utilised when required. An organ void of ducts, not intended for secretion, but for the elaboration and assimilation of germ-cells, the materials of which are derived from the blood, must necessarily suffer in an especial manner from the action of a poison like malaria, which directly tends to decompose this blood, from which it derives the materials for the performance of its function. (Dr. J. Sullivan, p. 48.)

SALICYLIC ACID AS AN ANTISEPTIC AND ANTIPYRETIC.—Up to the present time the antiseptic system of treatment as applied to the zymotic diseases has failed, more or less, for the reasons that either the drugs used have upon absorption into the system been so changed that their properties have altogether departed, or they have exercised such a poisonous influence upon the system that their administration has either been impossible, or being attended with danger and disagreeable results, it has been in such small quantities as to be practically of little value in the treatment of the disease. With salicylic acid, however, the case is different, its administration when pure in large doses is unattended with danger. When pure salicylic acid is administered, part of it passes down the intestinal canal unchanged and may be detected in the fæces, whilst the rest is absorbed into the blood: apparently in the form of the neutral salts of soda and potash, it is then eliminated by the kidneys and appears in the urine in the form of

salicylicuric acid. I prefer to produce a combination of salicylicate of soda and ammonia as in the following prescription: ℞. Sodæ bicarb. gr. v.; ammon. carb. gr. v.; acid salicylic gr. xx.; aq. ad. ℥i. (Mr. Engledue Prideaux, p. 59.)

SEPTIC POISON:—*Its Nature and Mode of Production.*—A poisonous septic fluid may be prepared by infusing animal tissue (*e.g.*, muscle) in water, and when this becomes putrid, boiling it with alcohol, removing the alcohol by filtration and evaporation, and extracting it with water. This fluid contains septic poison, capable of destroying life, yet whatever the poison be, it has not been destroyed by boiling alcohol. It has been proved by a well-devised and beautiful series of experiments that the poisonous fluid contains neither bacteria nor germs of any kind; the poisonous matter can, however, be filtered out by a porcelain filter. Now, having ascertained that the fluid containing the poison is free from germs, it is necessary to find out the source of the poisonous material. It arose from the putrid infusion of animal tissue, but how did it get there? It may be now considered as proved, by the observations of Dr. Burdon Sanderson and others, that this virulent poison is the product of a fermentation in the infusion of muscle, the fermentation being promoted by the presence of bacteria, just as the vinous fermentation is promoted by the yeast plant; the ultimate result in the former case being the septic poison—in the latter, alcohol, or, as we may fairly call it, the alcoholic poison. I must here mention that the bacteria, which first appear in organic infusions or cultivating fluids, do not possess the power of producing a septic poison, but the succeeding generations produced by these bacteria do possess this power. The first generation of bodies do not present the same appearance as those which follow, bacteria having the appearance of little rods, produce as offspring, not rod-like bodies, but spheroidal bodies, called micrococci, which have a tendency to collect into masses (glœa) in which the bodies appear to cohere by means of a gelatinous substance. So much then for the relation of fermentative processes to the production of disease, and it amounts simply to this—that certain fermentations generate poisons, which in their turn may produce diseased conditions. (Dr. T. W. Grimshaw, p. 3.)

TYPHOID FEVER.—*Oil of Turpentine and Lucca Oil in the Diarrhœa of Typhoid.*—Oil of turpentine combined with lucca oil, in the proportion of fifteen minims of the former to thirty or more of the latter for a dose, and made up with mucilage or the yolk of an egg, is the best remedy for the diarrhœa and tympanites which are generally associated with typhoid.

Under its influence the stools are reduced in number, and become more solid in consistence, and the tympanites gradually less. In only a few patients have I known its administration to be attended with disagreeable results to the stomach, the greater number manifesting a wonderful tolerance of it. It may be given three or four times in twenty-four hours, and continued for weeks if necessary. It prevents the palpable wasting of the disease, and obviates the necessity of giving alcoholic stimulants to a great extent—no mean desideratum in a workhouse institution. (Dr. P. L. O'Neile, Practitioner, June, p. 435.)

Pyrexia of Enteric Fever.—The pyrexia of enteric fever may be better treated by quinine and the bath, than by the bath alone. Where everything is favourable, and where the demands for treatment are not more frequent than twice or thrice a day, we may rest content with cold bathing. With the aid of quinine we may dispense with the heroism of four hundred baths in the course of one case of enteric, and we need not let a pyrexia remain untreated when failing heart's action or complications such as pleurisy, peritonitis, hemorrhage, forbid the bath. (Dr. J. Greig Smith, p. 41.)

AFFECTIONS OF THE NERVOUS SYSTEM.

CHOREA.—*Subcutaneous Injection of Curara.*—As curara had been recommended in hydrophobia, I suggested it might be tried in chorea, so solutions of the following strength were injected subcutaneously on the dates specified. Jan. 21st, 1878, one-twentieth of a grain; Jan. 22nd, one-twentieth of a grain; Jan. 26th, one-tenth of a grain; Jan. 28th, two-tenths of a grain; Jan. 31st, four-tenths of a grain. On Feb. 2nd four-tenths of a grain of curara were injected. Pulse (felt for the first time) 68, regular. The jactitations were now far less violent, and both the patient and his relatives remarked the improvement without any bad effect. Four-tenths of a grain were injected on Feb. 4th, 5th, 6th, 7th, 8th, 9th, and 10th, by which date he was perfectly quiet, able to walk without help, and could sleep at night without chloral. He had no other medicine at all but curara since the subcutaneous injection treatment was begun. Four-tenths of a grain were injected on the 14th and 18th; on the latter date he spoke a sentence, slowly, but plainly, and readily intelligible by listening attentively. On the 19th, four-tenths of a grain were injected, the patient being in remarkably good spirits. The same quantity was injected on the 22nd, on which day Mr. C. put on his spectacles and read his bible—the first time he had been able to do so for three years. From the last date he

has been kept quiet by injecting four-tenths of a grain of curara twice a week. The preparation used in Mr. C.'s case was by Messrs. Savory and Moore, in the form of discs of gelatine, a quarter of an inch square. (Dr. R. T. Wright, p. 76.)

HEAT APOPLEXY.—Have ice applied to the nape of the neck and head, and give fifteen minims of liquid extract of ergot and three minims of tincture of aconite every hour. (Dr. R. F. Dedricksen, p. 73.)

SANGUINEOUS CEREBRAL APOPLEXY.—*Subcutaneous Injection of Ergotine.*—The utility of the subcutaneous injection for the exhibition of the active principle of ergot, on account of the rapidity and comparative certainty of its action, has been most successfully demonstrated in cases of post-partum hemorrhage. From the explanation given of its inducing the contraction of the smaller arteries, and from the facility of its administration, especially in cases where swallowing is at least very difficult, I was led to use it in cases of cerebral apoplexy and also of hæmoptysis. For the success of this treatment, both temporarily and permanently, a great deal depends on the promptitude of its administration, before much hemorrhage has taken place and consequent damage to the cerebral substance. The strength of the injection I employ is ten grains of ergotine to the fluid drachm; injecting twelve minims deeply into the muscles, and not merely into the subcutaneous tissue, as in the latter case suppuration is very apt to ensue. (Dr. N. S. Foster, p. 74.)

SPINAL SCLEROSIS.—*Diagnosis.*—If a healthy man sits with one knee-joint resting upon the other (the very common attitude), and the ligamentum patellæ of the supported leg be smartly struck just below the knee-cap with the side of the hand, a sudden contraction takes place of the quadriceps femoris muscle (of which the ligamentum patellæ represents the tendon), and the foot is consequently jerked upwards in a degree which varies in different individuals. Now in confirmed examples of locomotor ataxia this reaction does not take place. No matter on what part of the ligament below the knee-cap, or with what force the blow is struck, the foot hangs motionless. In order to establish with accuracy the absence of the phenomenon certain precautions ought to be taken. The leg should be bare; the patient must not offer voluntary resistance to the movement of his leg, and the ligament should be struck with some hard implement which can be swung like a hammer. I find an ordinary wooden stethoscope answer very well if it is held loosely by the small end, and the blow given with the edge of the ear-piece. But, however administered, several blows should be struck on the

ligament, slightly changing the position each time, as there is generally one spot from which the reaction is peculiarly energetic. This is usually a little below, but very near to, the patella. (Dr. T. Buzzard, p. 68.)

Toxic Insomnia.—The external poisons which most frequently cause sleeplessness are tobacco, alcohol, tea, and coffee; the internal, certain effete products of tissue metamorphosis which accumulate in the bodies of gouty persons or of those whose kidneys act deficiently. Many a man does not and cannot sleep sufficiently because he smokes excessively. Cut off his cavendish or his cigars and he will sleep well. Many smokers know that they sleep badly if they smoke more than their usual quantity of tobacco, or if they smoke stronger tobacco than that to which they are accustomed. If a man who smokes two cigars every evening is induced at some time to smoke three, or if a smoker of bird's-eye ventures to replace it by cavendish, he may, when he has gone to bed, find he cannot sleep; and the cause of his sleeplessness is the smoking of more or of stronger tobacco than by habit he has hitherto borne without discomfort. Tobacco-smoking stimulates the cerebral circulation; it disposes to a succession of pleasing ideas by inducing an easy flow of mental activity. But this stimulation of the blood-flow in the brain is sure, if pushed to undue limits, to induce cerebral vaso-motor debility or paralysis, and, as a consequence, persistent conscious thought. And so, *mutatis mutandis*, does alcohol cause sleeplessness. The man who drinks to commencing drunkenness mostly sleeps soundly, if not well. But many a so-called moderate drinker knows that he sleeps badly if he takes a little more than his usual quantity of wine after dinner, or even his usual quantity of some unusual wine. Alcohol flushes and dilates the smaller blood-vessels, especially those of the brain; if such a condition be maintained sleep is disturbed or wanting. There are varieties of toxic insomnia which are apt to occur in gouty persons, or in those whose kidneys are failing, and which arise from the accumulation in the blood, in consequence of deficient excretion, of the products of tissue-metamorphosis. Insomnia of this kind is rarely complete. But the patient may complain that he sleeps very badly, that he lies awake for some hours and has great difficulty in getting off to sleep, that he is easily awakened and wakes frequently, and that he always dreams when he sleeps. In such a case we may find a pulse of high tension; the aortic second sound may be accentuated, and the first sound of the heart may be reduplicated at the apex. Where there is chronic renal disease, we may also find the

direct physical evidences of the characteristic cardiac hypertrophy which accompanies chronic interstitial nephritis. I believe that insomnia in such cases is due to the maintenance of a state of high tension in the cerebral arteries. I wish to impress upon you that we find the clue to many cases of sleeplessness in the signs of the gouty diathesis or in the discovery of albuminuria. Again, there is a senile form of insomnia. You may perhaps have observed amongst your friends that an exaggerated appreciation of the merits and value of early rising mostly increases as age advances. The sleeplessness from which many old persons suffer is mainly, if not entirely, the result of senile degeneration of the smaller cerebral arteries. Those vessels are less elastic and less contractile than in health, and their weakened walls often lead to their permanent dilatation; they are physically unable to adapt themselves fully to the condition of relative arterial anæmia which is requisite for healthy sleep. The tendency of this condition of the blood-vessels of the brain to prevent or diminish sleep is probably to a great extent counteracted by the cardiac feebleness which so frequently and so fortunately coexists with the vascular changes. In the treatment of insomnia we must often use soporifics. Of these the chief are chloral, opium, morphia, the bromides, Indian hemp, alcohol, and affusion with cold water. The successful treatment of a case of sleeplessness follows from the discovery of its cause. (Dr. J. Sawyer, p. 84.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

GOUTY HEART.—The dietary should contain but a small proportion of albuminoid material—indeed just so much as will meet the tissue requirements of the body, and no more. And that is much less than is usually imagined. Every particle of nitrogenised material taken in excess of the tissue-needs is so much more waste matter to be dealt with—so much more added to the excrementitious matter which has to be got rid of, or which, if not got rid of, will do definite and intelligible mischief. We have abundance of evidence to show that not only does nitrogenised waste raise the blood-pressure in the arteries; but we know from the observations of the late Prof. Parkes at Netley, that a strictly non-nitrogenised dietary notably lowers the blood-pressure. Consequently, if the gouty patient be placed upon a non-nitrogenised dietary, all these grave and serious consequences of persisting lithæmia will be delayed and kept off for years. The patient should be put upon a dietary which should consist of farinaceous material, fruit, fat and fish. Fish of course contains nitrogen like all other flesh, but a gouty person may eat a good meal of

fish, and enjoy it, without getting any great excess of azotised material. Then he should take a good amount of vegetables of all kinds—in winter, potatoes, turnips, carrots, winter greens; in spring, spinach, but not much asparagus, for that vegetable contains a quantity of oxalates which tease a gouty system annoyingly. Consequently, rhubarb is to be avoided; and salads, which should form a portion of the daily dietary of the gouty all the year round, as far as possible, should never contain sorrel, the *Rumex acetosus*, on account of the the oxalates present in it. Greens of all kinds, except those just mentioned, cooked and uncooked, should form a considerable portion of the dietary. A lobster salad is the typical food of the gouty man whose digestion is perfect. Potash, or lithia, and the acid phosphate of soda, all form uric acid into soluble salts. They may usually be given persistently without harm. Some persons are very susceptible to the depressant action of potash, which as a muscle-poison lowers the activity of the heart, and consequently cannot take it in any quantity. But most gouty persons are all the better in every way for the steady use of potash, with or without a little lithia, and can take it freely without inconvenience. It may be given with colchicum or buchu; buchu forming an excellent vehicle. Each dose should be taken about half an hour before a meal, and be washed down with a tumblerful of water. The water is a therapeutic agent of no mean value in these cases. It dilutes the potash and washes the tissues. The first dose ought to be taken on getting out of bed, so that the alkaline fluid may bathe the tissues before any food is taken. Potash, by getting rid of the exciting agent, the uric acid, permits the circulation to quiet down to its normal point. The dyspepsia of gout yields also to a course of alkalies. Exercise and improved oxidation are useful adjuncts. Exercise does not tax the heart injuriously so long as its tissues are sound; neither does it provoke palpitation, as is the case with the muscularly feeble heart. The removal of waste by increased oxidation is a desirable matter. By such means the gouty heart may be long preserved and maintained, and the risks of apoplexy and angina avoided. As to angina, the inhalation of amyl during the attack is certainly indicated. Nitrite of amyl dilates the peripheral vessels, whose contraction is the starting-point of the attack of angina, and so lowers the blood-pressure in the arteries and relieves the heart. (Dr. J. Milner Fothergill, p. 329.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

ASTHMA.—*Nocturnal.*—The air of bedrooms, especially in the winter, proves, perhaps, the most frequent cause of the "noc-

turnal" asthma. Notwithstanding the diminished energy of all the vital changes during sleep, at least one thousand cubic feet of air pass every hour through the lungs, and return from them charged with more than four per cent. of carbonic acid, and completely saturated with water vapour; and the expired air contains ammonia, probably from decayed teeth, or from particles of food decomposing in the mouth, also hydrogen, hydrocarbons, and sulphuretted hydrogen which in consequence of a faulty digestion may diffuse themselves into the intestinal veins, and be eliminated by the breath. The products of perspiration and those derived from the combustion of candles, lamps, or gas, contribute in their turn to increase the insalubrity of the place. It is known that the air which contains one per mille of carbonic acid is irrespirable, and its injuriousness is due, not so much perhaps to that gas itself—which when pure, may without harm indeed be inhaled in a somewhat larger quantity than is then present—but to the organic substances which always accompany it. Yet even a slight excess of the pure carbonic acid causes turgescence, a sensation of heat and pricking in the conjunctival and respiratory mucous membranes, while at the same time it increases the cutaneous and mucous secretions. In the presence of organic substances, however, a much smaller quantity of it becomes highly irritative. However, large a bedroom may be, a few hours' occupation of it will render it insalubrious in the manner just mentioned, unless the air be constantly renewed to the extent of at least two thousand cubic feet per hour and person. How imperfect even such ventilation must be, as regards the removal of the gaseous impurities, in comparison to their dilution out of doors, is evident from the fact that here more than six millions cubic feet of air are available for the purpose. Unfortunately asthmatics, as a rule, adopt no measures for the supply of fresh air. (Dr. J. B. Berkart, p. 109.)

PHTHISIS.—As regards consumption we must look in the capillaries for the first radical changes which produce all the varieties of that disease, the defect consisting essentially in defective mineral nutrition. In advanced cases is another agency which seems to exert considerable influence in preventing recovery: septic influence. Disintegration of tissue brings the effete products of tissue destruction within reach of the absorbents or of the blood, and so has a tendency to vitiate the blood. No one doubts this in cases of advanced cancer; is it otherwise in phthisis? The general emaciation shows that the whole system is affected by the disease; the nutrition of the whole body is interfered with. Such cases may be confounded with cases of starvation. In consump-

tion and other wasting diseases, the capillaries are underfed, and cease to do their work. This may arise either from deficient supply or from non-assimilation of mineral matter. It is the starting-point of the disease, and life ebbs out slowly. Cod-liver oil, being by many persons easily assimilated, supplies fuel at the right time, and so far lightens the work of the underfed capillaries, so that what remains of their force may be turned to other purposes of nutrition. But cod-liver oil does not contain the elementary principle which the chemistry of consumption shows to be deficient. Dr. Anderson proposes to use a tribasic phosphate containing all the bases which analysis has discovered in the coats of the aorta and all the organs examined? He proposes, by supplying all the mineral ingredients of nutrition, to leave nothing to chance, so that if there be a fair amount of assimilation, all the mineral elements for tissue-fabrication or renewal may be present. (Dr. M. F. Anderson, p. 337.)

Antiseptic Atmosphere in Phthisis.—For many years I have held that the majority of phthisical patients die of septicæmia, and that the arrest of this daily re-poisoning is a primary object of treatment. To reach, cleanse, and dress ulcers of the lungs by surgical methods seems impossible, and the effects of antiseptic inhalations are disappointing; if, however, there be an antiseptic climate, we may hope to counteract this secondary blood-poisoning by sending our patients to live in it. Such antiseptic or aseptic climates are found in Switzerland, in Upper Egypt, and in other districts; and in the mountain air of Switzerland there is found also a tonic virtue which no doubt largely aids the physician in his work. The aseptic state of the air on alpine heights has been proved by Professor Tyndall's experiments at the Bel Alp; but the Bel Alp, in common with most other Alpine resorts, is insufficiently sheltered. In Davos alone, so far as we yet know, is found an air at once aseptic, bracing, and still. Davos is situated in an Alpine valley towards the east of Switzerland, in the Grisons. There is good accommodation. (Dr. T. C. Allbutt, p. 95.)

Early Phthisis.—When the tongue is covered with a thick fur, it is useless, or nearly so, to give iron and cod-liver oil; for the tongue is the indicator of the state of the intestinal canal, and absorption through the thick layer of dead epithelium cells is well-nigh impossible. It is well here to give a compound calomel and colocynth pill every second night, and to prescribe a mixture of nitro-hydrochloric acid, or phosphoric acid with infusion of cinchona, three times a day, till the tongue cleans. Or at other times the tongue is raw, bare, and denuded of epithelium. Here it is of cardinal impor-

tance to put the patient on a mixture of bismuth with an alkali, and a milk dietary. Often milk and seltzer-water will agree where milk alone is too heavy and too constipating. As long as the tongue is raw it is necessary to fight the case on this line; attending to the night-sweats of course, but not attempting to give hæmatics or oil. Diarrhœa should always be attended to energetically. Of course in the later stages where the intestines are the seat of tuberculous ulceration the diarrhœa is very intractable, requiring the free exhibition of bismuth and opium, and even of ipecacuanha, which seems to be of service in such cases. But in the early stages it will yield to a pill of sulphate of copper half a grain, and extract of opium one grain. (Dr. J. M. Fothergill, p. 92.)

Night-Sweats.—If asked the question, "What do you think the most important matter to attend to in the treatment of early phthisis?" my answer would be "To arrest the night-sweats." To arrest night-sweats we must have recourse to some anhidrotic, as oxide of zinc, sulphate of copper, or one of the solanaceæ, as hyoscyamus, and still more, belladonna. Probably hyoscyamus acts in an allied manner. Taken altogether there is no anhidrotic to be compared with belladonna: though in the few cases where it fails the other agents may be tried. But in order to get out the good effects of belladonna, it is necessary to give it in sufficient dose. The ordinary dose of sulphate of atropia—for it is much better to use a solution of atropia of known strength than to give the tincture of belladonna, which may, and probably usually does, vary in strength—is in many cases quite insufficient. I use atropia in doses varying from the seventy-fifth (75th) to the fiftieth (50th), and up to the twenty-fifth (25th) of a grain. A considerable proportion of patients are unaffected until the last dose is reached; and even then do not complain of much dryness of throat, or indistinctness of vision (effect upon the pupil as a guide to the administration of belladonna is utterly worthless). With many patients the seventy-fifth of a grain of atropia will arrest the night-sweats, and in a certain number will affect the throat and eyesight; while others require the fiftieth to influence the night-sweats: and again a small proportion are uninfluenced till the twenty-fifth is reached. (Dr. J. M. Fothergill, p. 89.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

Acid Dyspepsia.—The effect of bicarbonate of potash, taken after food, on the acidity of the urine, is different from that when it is administered before meals. For when taken on

an empty stomach we have seen that the acidity on the day of administration was only slightly depressed, whilst on the day following the acidity was considerably higher than it was the day before the salt was taken. But when it was administered during the process of digestion the acidity of the urine entirely disappeared, being on two occasions neutral, and on one alkaline, whilst on the succeeding days there was no marked increase in the acidity of the urine as compared with that of the days preceding the experiment. And the same difference is observable in the hourly variations of the urine, for when the bicarbonate was taken before meals the effect of the alkali passed off at the end of two hours, and the amount of acid passed in the succeeding three hours was nearly equal to what was passed on the day no medicine was taken; whilst when the salt was taken after meals the urine remained alkaline up to the end of four hours after the dose was taken, and no recovery of acidity was noticeable. The therapeutic indications to be drawn from these observations may be thus summarised: 1. In cases of acid dyspepsia arising from the excessive formation of acid within the system, as in lithæmia, the alkaline bicarbonates should not be administered before food, but after. 2. The administration of alkaline bicarbonates before meals is indicated in those cases where the free acid is formed in the stomach itself, the result of fermentative changes of undigested food or morbid mucus, when it is necessary to diminish the too high degree of acidity thus caused in order to permit digestion to be properly performed. (Dr. C. H. Balfe, p. 345.)

CHRONIC DIARRHŒA.—*Cold Water Injections.*—Dr. Messemer, of the Mount Sinai Hospital, New York, relates three cases of the value of his practice in the treatment of very obstinate chronic diarrhœa, accompanied by much tenesmus. He orders a quart of iced water to be injected (in divided doses of half a pint) after each stool, the exit of the water being assisted after each injection by pressure made on the abdomen by the hand. After the expulsion, a small quantity (about half a teacupful) may be again slowly injected, so as not to excite peristaltic action; and this is enough to keep the walls of the rectum apart and prevent their irritating each other. He does not object to internal treatment being employed at the same time, and he considers proper dieting as of the highest importance. Improvement soon resulted in these cases, although they were examples of bad diarrhœa. (Dr. Messemer, Med. Times and Gazette, Aug. 31, p. 249.)

DIPHTHERIA.—Dr. Bell, of Glasgow, has been very successful in the treatment of diphtheria. The following illustrates his

plan of treatment. Two children, aged 10 and 8 years respectively, were both prostrated on the same day. The film was spreading very rapidly over the tonsils, and there was considerable fever. The throat of each patient was freely painted every two hours with the following application: glycerine of carbolic acid, the strong liquor of the perchloride of iron, and sulphurous acid, of each three drachms; and the following mixture was given: chlorate of potash, two drachms; sulphurous acid, two and a half drachms; glycerine, one ounce; and water, to four ounces: two teaspoonfuls to be taken every two hours. In this way, while the patients were awake, an antiseptic was brought into contact with the diseased surfaces every hour. Port wine, soups, and milk were also given freely. The tonsils had only been painted a few times when the film showed signs of ceasing to spread; and within four days all the diphtheritic membrane had disappeared, and saving a considerable amount of prostration, the patients were pretty well. A course of tonic treatment, however, soon removed this debility. (Dr. R. Bell, p. 123.)

[We have tried this plan in one case, but the patient, a man about 35 years of age, could not bear the repetition of the application. The throat was well painted over, as directed by Dr. Bell, a few hours after the appearance of the film, which was then rapidly spreading. On the next inspection of the throat not a trace of the film was to be seen. As the patient objected to the re-application of the liquid, owing to the suffering the first had caused, it was not again used. The medicine was continued and rapid recovery took place.—EDS.]

FISTULA IN ANO.—During the last five years I have had under my care several cases of fistula in ano. All but the last were subjected to the division of the sphincter ani with the knife, and, although varying considerably in the length of time required to heal the wound, made good and perfect recovery. With the last I adopted a different method. It was a complete fistula, having one opening in the bowel above the sphincter, and another (external) near the anus, whence frequently fæcal matter exuded. Having cleared the bowels thoroughly, and then secured them against disturbance for two or three days, I introduced to the bottom of the fistula in a hollow tube some pure carbolic acid in a liquid state. The first application seemed to close the internal opening; the second, introduced the same way, required a less deep insertion; the third less still, and so on till the sinus became completely closed. Two months have now elapsed. The

patient has had no return of the affection, and declares himself perfectly well. (Dr. W. H. Lambart, Liverpool, Lancet, June 8, p. 854.)

HERNIA.—*Operation for the Radical Cure of Inguinal Hernia.*—The patient lying on his back with the knee slightly bent and raised, is put completely under the influence of chloroform. An incision an inch and a half long is made obliquely across the scrotum about three inches below the external ring. This incision divides the skin and dartos alone. After a few touches of the knife,—the finger-nail, and then the handle of the knife, can be used to separate the skin and dartos from the hernial sac and its denser coverings (which for convenience I shall call the *sac*). This separation is to be made all around the incision to the extent of a five-shilling piece or more, according to the size of the tumour. The point of the forefinger is now to be put through the incision, against the lowest part of the sac, and, by a gentle shaking and pushing motion, is made to invaginate the sac (without the skin), up through the external ring into the inguinal canal, as far up as the internal ring. The finger is now lying in the canal in front of the cord. The tip of the forefinger is now shifted underneath the inner pillar of the external oblique aponeurosis, and by a little manipulation can hook forward below that aponeurosis, the lower edge of the internal oblique just where it ends in the conjoined tendon. This can readily be felt as a prominent ridge against the external aponeurosis. While the tip of the finger is retaining the parts in the position above described, the surgeon takes in his other hand a very strong curved needle with an eye-hole near the point, and fixed in a strong handle. An assistant has near at hand about two feet of thick, strong, pliant silver wire bent double, so that the two ends are together, about half an inch of each end being bent into a hook. The curved needle, well oiled, is now entered at the scrotal incision, slipped along the inner edge of the forefinger as far as its tip—that is the edge nearest the umbilicus—and is made to pierce the conjoined tendon, also the internal pillar, so that its point projects below the skin not far from the situation of the internal ring. The skin is now slipped over the subjacent structures as far as it can be drawn towards the umbilicus and the needle point thrust through it. One of the hooked ends of the silver wire is now put through the hole near the needle point, and by a rapid motion the needle and wire are drawn down along the edge of the forefinger and out of the scrotal incision. The needle is again entered as before and passed along the outer edge of the forefinger, and by it guided underneath the outer pillar of the external ring as far up as the

former puncture. The needle is now made to pierce the aponeurosis near Poupart's ligament, and its point rests against the skin, raising it up. The skin is pulled downwards and outwards, till the former puncture is opposite the point of the needle, which is then pushed through it. The other hooked end of the wire is put through the needle's eye, and by it pulled down the canal and brought out at the scrotal incision. The next step is to pinch up the sac from off the cord, at the situation of the external ring, with the forefinger and thumb. The unarmed needle is now put in at the scrotal incision and passed through the sac, across the cord, below the edge of the internal pillar through which it is thrust, and made to project at the inner angle of the scrotal incision when it is pulled upwards to be opposite the needle point. The hooked end of the wire which was first drawn down is now put through the needle's eye, and by it drawn out at the point where the needle was inserted. An assistant puts his finger into the loop of wire which remains protruding from the hole in the skin of the groin, which was made by the punctures, and holds it fast, while the operator takes the ends hanging out of the incision in the scrotum and pulling them firmly straightens the wires as they lie in the canal. This stretching, done with some force, draws the two sides of the canal together, so that the external ring is tightened till it grasps the cord. The inclusion of the conjoined tendon, or lower border of the internal oblique, in the grasp of the wire, secures that all the boundaries of the canal are drawn closer. The two ends of the wire in the scrotal incision are twisted firmly together up into the canal, and the loop at the groin is twisted down into the puncture, in each case three turns in the same direction being given to the wires. By this the sides of the canal are still further approximated, and part of the tissue entangled in the twist of the wires. The loop of the wire is now bent down to the skin and the wires with the two free ends bent up and passed through the loop and secured; pressure of the wires on the skin being prevented by a pad of lint. A little oiled lint is placed on the scrotal incision, a pad of lint and compress, retained by a bandage, completes the dressing. (Dr. G. Buchanan, p. 184.)

INTERNAL HÆMORRHOIDS.—*Glycerine.*—Try glycerine, giving it in one drachm and a half doses every three hours. After taking this for twenty-four hours, the patient's pain was much relieved, and after forty-eight hours it was entirely gone. I may say, however, that the cure was not permanent after leaving off the medicine, as he has had several attacks

of pain since, but never so severe as before. He keeps some of the glycerine mixture by him, and a few doses always have the desired effect. (Mr. W. H. Jalland, British Medical Journal, June 29, p. 934.)

INTESTINAL OBSTRUCTIONS.—I would submit the following propositions with reference to the tapping of the intestine: 1. That there are cases of obstruction in which tympanitic distension of the abdomen is one of the most urgent symptoms. 2. That whether these be acute or chronic, relief is afforded by tapping with a fine trocar. 3. That if due precaution be taken no evil results, such as peritonitis, sloughing, septicæmia, &c., need be anticipated from the operation. 4. That especially in cases of rapid distension, where accumulation of fæces has not been going on for a lengthened period, good is likely to result from the operation. 5. That permanent relief may follow the operation to such an extent as to entitle it to be considered sometimes a curative means, obstruction being removed and not recurring afterwards. (Mr. W. W. Wagstaffe, p. 128.)

LIVER AFFECTIONS.—*Sulphate of Manganese.*—This is most useful in cases of hypochondriasis and hepatic dropsy. Half a drachm is the utmost limit, and, saving in cases of urgency, by no means desirable. From ten grains to a scruple will produce large bilious evacuations. Ten grains will rarely be attended with nausea, and is sufficient for ordinary purposes. I have known it often to relieve hæmorrhoids, and those congestions of the fauces and bronchia so common in our daily practice; in fact, wherever we can trace these congestions to a defect in the portal circulation, the manganese is a good substitute for calomel, as it produces no more irritation to the intestinal or anal glands than is caused by a flow of bile, and it is attended with neither griping nor depression. Five-grain doses may be repeated as an alterative instead of blue pill or grey powder, and it is remarkable that many anæmic patients who cannot take any of the preparations of iron can do so if combined with a few grains, say from two to five, of sulphate of manganese. I find the best way to administer it is to dissolve ten grains in a tumbler of water, and then add some citrate of magnesia or potassa to the solution, and drink in a state of effervescence. The taste is not unlike that of Epsom salts, but not quite so bitter, and is scarcely perceptible when taken in a tumbler of effervescing mixture. (Dr. R. H. Goolden, p. 341.)

PARACENTESIS ABDOMINIS AND THORACIS.—*Drainage by Capillary Tubes.*—Just in the same way that an anasarcous limb may be drained of fluid by means of a fine Southey's cannula

and capillary india-rubber tube, so ascites may be treated. The trocar and bulb-headed cannula required for the purpose of gradually drawing off ascitic fluid, by the help of a capillary tube, differs very little from that employed by me for anasarcous limbs. Both instruments are equally fine. The calibre is the No. 1 exploring trocar of the surgeon. One long needle-trocar, measuring an inch and three-quarters in length from hilt to point, has appeared long enough for all the cases hitherto tapped by me. Three or four cannulas of different lengths, adapted to the thickness from fat and œdema of different abdominal walls, are required. The cannulas may be perforated with as many as six or eight side holes—the more the better, so that their strength is not interfered with. The mouth end of each cannula should be armed with a small silver plate or shield, to obviate any risk of the cannula head sinking beneath the surface of the skin when this is highly œdematous. Trivial as this is, it appears to me from experience that there is a right and a wrong way of introducing the cannula. Instead of driving the trocar in quite perpendicularly, it is best to slope the point downwards somewhat towards the pubes, and to avoid making the cannula point upwards towards the sternum. The wound made is so slight that one can afford to make it almost anywhere, but from prejudice I should select the raphe or mesial line below the umbilicus, and about midway between this and the pubes. (Dr. R. Southey, p. 193.)

Dr. Goodhart relates four cases of paracentesis by means of Southey's capillary tubes. This method of performing paracentesis abdominis will be sufficiently clear from a perusal of the cases. Dr. Southey's proposal for the treatment of anasarca is borrowed and applied to the treatment of fluid in the abdominal cavity—that is to say, a cannula of very fine bore is passed into the peritoneum, and, by means of a long drainage-tube attached to it, the fluid is slowly drained away. In the first instance the tubes made for the purpose of draining the subcutaneous cellular tissue were used, and they answered very well in that particular case; but, doubting whether they would in all cases be long enough, Mr. Hawksley made me some of additional length—one inch and a half, —but in all other respects after Dr. Southey's pattern. These have been in use since. The tubes have been retained in the abdomen for varying periods, but generally for about twenty-four hours, and it will not often be necessary to continue the drainage for a longer time than that. Should it be otherwise it would probably be better to withdraw the tube and insert another at some other spot. Besides these cases of

ascites, I have treated in a similar way a suppurating hydatid cyst in the lower part of the abdomen, and an empyema. Both drained for a few hours very well and the empyema was probably thoroughly drained; the hydatid cyst, however, ceased to run, and other measures became necessary. It is not to be expected that purulent fluids can often be treated advantageously by drainage through very minute tubes, and particularly in the chest, where the pus is liable to be thick. Still cases do occur in which such a plan is applicable, and there can be no harm in testing this as a preliminary measure. In pleuritic effusions of serous character slow drainage of this kind has, I think, several points in its favour, as compared with aspiration; and in peritoneal effusions, in which we can calculate with considerable confidence upon the limpid nature of the fluid, the advantages of paracentesis performed in this way are sufficiently obvious to make it a wonder that it is not more usually adopted. (Dr. J. F. Goodhart, p. 191.)

RECTAL FISTULÆ.—*Immediate Ligation.*—Some time since I adopted a method at the Hospital for Women, for the treatment of rectal fistulæ which, so far as I can ascertain, is new. It consisted in passing a strong and well-waxed silk ligature along the track of the fistula into the bowel. An ordinary surgical probe, with an eye at its end, carried this thread into the rectum. My bivalve expanding speculum was previously introduced, and by its use there was no difficulty in seeing and seizing the ligature and bringing it out through the anus. The probe was then withdrawn, and the ends of the silk were wound round two strong pieces of wood, which were held between the fingers of each hand. An assistant passed a finger on either side of the track of the fistula, to steady the tissues, and to resist the traction which was put on the silk thread. The two pieces of wood were then drawn towards me with a rapid sawing motion, and the fistula was quickly divided with the loss of scarcely any blood. Some oiled lint and a pad and bandage were applied in the usual way, and the wound healed well. No anæsthetic was administered, and although the patient did not relish the operation, still it was quite bearable, and what she felt most was a burning sensation, due doubtless to the friction of the silk. Since then I have rapidly divided a fistula which ran rather high up, by passing a strong piece of waxed twine, and tying the tissue till it was divided. This is somewhat easily effected by drawing on the ends of the twine until the parts included in the knot have given way. The vessels are crushed before they are torn through, and no blood, or the merest drop is lost. (Mr. H. A. Reeves, p. 188.)

AFFECTIONS OF THE URINARY SYSTEM.

DIABETES.—*Origin of the Sugar of Diabetes.*—The liver is essentially a sugar-assimilating instead of a sugar-forming organ; and when its assimilative action is properly exerted, so little sugar is allowed to pass into the general circulation that the quantity existing in arterial blood is insufficient for rendering the urine more appreciably saccharine than is observed in the healthy state; but when its assimilative action is not properly exerted, sugar is allowed to pass, and in proportion as it does so the urine acquires a more or less marked saccharine character. We know that in a large number of animals the food is of such a nature as to supply sugar in notable quantity for absorption from the alimentary canal. A portion of this may reach the thoracic duct through the absorbents, and thence be conveyed to the general circulation, accounting in part for what sugar is there encountered. The main channel, however, for the passage of sugar from the alimentary canal appears to be the blood-vessels, and transmission into them is permitted by the property of diffusibility which this agent possesses. Absorbed into the portal system, it is conveyed to the liver, where it becomes, if not entirely, certainly almost entirely, checked in its onward progress, and prevented from entering the general circulation. (Dr. F. W. Pavy, p. 134.)

PARALYSIS OF THE BLADDER.—*Hypodermic Injection of Ergot.*—M. Suton, in a paper read before the Société Médicale de Reims, advocates the use of a tincture of ergot for hypodermic administration. There is, says M. Suton, no occasion to show that ergot acts chiefly upon the organic muscular fibre of the abdominal viscera—the uterus, bladder, rectum, &c. We wish to show the special application of that drug to the treatment of retention of urine of a functional or paralytic nature. The efficacy of ergot in such cases was strikingly shown by an observation recorded in Le Mouvement Médical, for while that drug was being administered in a bad case of typhoid fever, with the view of arresting an intestinal hemorrhage, accompanied with distressing meteorism, the bladder, which was paralysed and very much distended, was, at the same time, freely emptied. It is not in one case merely, but in several cases, that this treatment has been successful, the urine being generally passed about half-an-hour after the injection. It is particularly serviceable in retention of urine of a simple paralytic nature, and especially in cases of adynamia, coma, profound prostration, &c. (M. Suton, Med. Press and Circular, May 29, p. 453.)

STONE IN THE BLADDER.—*The Microphone as an aid to detecting.*—As soon as I heard of the microphone, which may be described as an instrument capable of greatly magnifying the smallest note, it naturally occurred to me that it would afford an opportunity of producing easily-attainable and unmistakable evidence of the presence of a foreign body of very minute size in the bladder. I went to the well-known inventor, Prof. Hughes, with this idea, and he at once most courteously provided me with a form of microphone which he believed suitable to the purpose, and explained the arrangements necessary. These consist in the production of a galvanic current, commencing in this instance with three cells of a Leclanché battery, and passing by means of a very small flexible insulated wire through a telephone, then through the microphone attached to the handle of the sound to be employed, and lastly back to the opposite pole of the battery to complete the circuit. When I say that this particular microphone which I employed is sensitive enough to make audiblethe march of a housefly over a piece of net, it will be understood that the smallest piece of stone, if struck, will certainly reveal its presence by a note. By the use of the microphone you can make absolutely, logically certain the existence of small fragments in the bladder, for the detection of which you have hitherto depended upon your unassisted ear and hand. I want particularly to say that the unassisted ear and hand will suffice for almost all cases. There may be one in twenty cases in which it may be necessary or desirable to use this instrument. It is something like the case of the endoscope in regard to the urethra. When it was first introduced, a good deal was said about its being applicable to all disorders of the urethra and the bladder; but it was found in practice that, however well it looked upon paper, there was only a case here and there that might be benefited by its use. (Sir H. Thompson, p. 198, and Lancet, June 1878.)

STRICTURE OF THE URETHRA.—The old practice of dilating a stricture by simple catheterism is the most useful, the most simple, and the most sure of all the plans that can be adopted. I may say, judging from the experience of many years past, both in hospital and private practice, that I have scarcely ever seen the necessity of having recourse to such measures as external or internal urethrotomy. The latter operation I have never performed. The several advocates of these various operations may perhaps reply that the experience of any one man is limited. Yet surely the practice of a large hospital should offer an opportunity, now and then at any rate, of performing some cutting operation. All I can

say is that I have only once performed section through the stricture, or, as it is called, external urethrotomy, during the last seven years, and then I regretted it afterwards. The plan of treatment I adopt is as follows:—A patient applies for retention of urine and no catheter can be passed. He is placed in a warm bath, and a good dose of opium is given. In many instances no other measures are necessary. And if this holds good for the relief of retention of urine, which, although dependent in part upon paralysis of the muscular coat of the bladder, is certainly in part also, in most cases, dependent upon a stricture, it might be expected that the same treatment would relieve the stricture itself, and this is really the case. Therefore, *in all cases*, if upon admission no catheter can be passed by the most moderate manipulation, the treatment above-named is continued, coupled with a reduction of diet and a complete absence of stimulants. No further instrumental interference is attempted for some four or five days, and at the end of that time it is rare to find any stricture resisting the passage of a small-sized gum elastic catheter. I scarcely ever use any other form. Should I even then be unable to pass an instrument, still I wait; the same treatment is continued, and a smaller catheter is afterwards employed. Sooner or later this is successful, and when once the catheter is introduced into the bladder it is tied in and urine allowed to run away through it, or, as very often happens, by its side. Notwithstanding all that has been said and written and done in favour of other methods, this is the one which hardly ever fails, but its successful application requires not only skill but patience and practice. Any rough usage to the canal will destroy the chances of success, and it will be no waste of time on the part of the surgeon if half an hour or an hour even be occupied in the most gentle manipulation, if at the end of that time a catheter is passed. Once in the bladder the battle is over, and the successful issue of the case is in the surgeon's hands. (Mr. J. Cooper Forster, p. 205.)

The urethra, even though absolutely impervious to the passage of the catheter from a combination of causes—viz., the stricture growth engorged with mucus and blood, and rendered painful by futile catheterism, and certainly spasm, is not absolutely impassable. The tightest part of stricture is that in front. The unconditional use of a catheter would, in such a state of the parts, certainly intensify the difficulty by calling into play a new source of resistance, in the form of normal muscular antagonism, to its passage—a force that is ever on the alert to oppose the enforced passage

of a foreign body through the urethra into the bladder. This automatic force can be brought under complete control by an act of volition, and not only so, but be made to impart to the strictured canal the greatest amount of patency and passivity of which it is capable. The means to this end consist in making the patient bring the sphincters or detrusors of the bladder and urethra into a state of absolute rest by voluntarily, but gradually, calling into powerful action their antagonists, the expulsors or accelerators, and using the catheter whilst the force thus elicited is kept in a state of strain. This mode of palsying the detrusors has another advantage which anæsthesia does not possess, since it assists the surgeon by employing the urine as a dilator, and thus reduces the resistance of the stricture slit. (Mr. J. Gay, p. 343.)

AFFECTIONS OF THE BONES, JOINTS, &c.

Actual Cautery in Surgery.—All objections to the use of the actual cautery vanish so soon as it can be said that it can be used without giving pain. For the last twenty-five years I have been in the habit of sealing bleeding vessels with the cautery, and have of late used it occasionally in operations of all kinds. The actual cautery has the great advantage of closing the veins as well as the arteries, and thus removes one of the gravest sources of anxiety to the surgeon, inasmuch as the great danger in surgical operations arises from the absorption of septic agents through unclosed veins. The ligature of a vein, as is well known, is not unattended with risk, for the obvious reason that during the time that the ligature is separating a certain amount of suppuration necessarily takes place, and the products of that action may give rise to septic absorption. The actual cautery, therefore, possesses a great advantage over the ligature, as far as the veins are concerned. The danger of blood infection, though in a much less degree, obtains when an artery is tied. In a vein the current of blood is towards the centre of the circulation, and in an artery from it. When mischief arises from the cause mentioned, after an injury to an artery, the effects are usually seen in the peripheral circulation. When venous absorption takes place, the results are generally manifested in the central organs. I have now used the actual cautery in a great variety of operations, and during the last twenty-five years in a large number of cases of hemorrhoidal tumours, but as yet have had no case of purulent absorption following its use. Another, though minor, advantage of the cautery is that it can be applied much more quickly than the

ligature; and this is by no means unimportant when many vessels have to be dealt with. Practically, it is difficult not to use the cautery too hot, as if it kill the parts to which it is applied the desired result is not produced. A good test as to the cautery being at the required heat may be obtained by dabbing the iron on a moderately wet sponge until only a little steam is generated. (Mr. Henry Lee, p. 348.)

AMPUTATION OF THE THIGH.—*Stokes's Supra-condyloid Method.*—The supra-condyloid amputation by the method of Stokes has not received sufficient attention in England. It is often confused with that amputation which goes by the name of Gritti. Between the two there is this all-important difference; in Gritti's the section of the femur is made through the condyles, in Stokes' at least half an inch above them, in other words the one operation is trans- the other supra-condyloid. The difference is very important, because in Gritti's operation the patella is either drawn up altogether on to the front of the femur, or projects forward like the half-open lid of a box, and requires considerable pressure to make it assume a flat position against the divided surface of femur. Stokes's operation, besides this, enables the patient to bear his weight better upon the stump; there is less risk of shock during the operation; and only one large artery requires ligation. In Stokes's operation, as in Gritti's, the medullary canal is not opened. (Mr. W. H. A. Jacobson, p. 142.)

ANTISEPTIC DRESSINGS.—*Thymol as a substitute for Carbolic Acid.*—The employment of thymol as a substitute for carbolic acid in surgical dressings is a natural consequence of the discovery of its greater antiseptic and less septic power. A lecture on the subject has been published in Volkmann's series by H. Ranke, of Halle. The solution used instead of the 3 per cent. solution of carbolic acid consists of one part thymol, ten parts of alcohol, twenty of glycerine, and a thousand of water, and can be employed as either a spray or a solution. An impregnated gauze is also used. Since thymol does not irritate the wounds, the gauze may be laid directly upon it; otherwise the same method is employed as in Lister's plan. If the gauze becomes hard and dry it may be moistened once or twice a day with thymol water. In order to prevent the evaporation of the thymol from the dressing, the gauze is covered with oiled paper. From an experience of forty-one wounds dressed with thymol the lecturer concluded that the method leaves nothing to be desired as to its antiseptic effect, and that it answers better than the carbolic acid dressing, since the secretion from the wounds is less, the period of healing shorter, and the cost of the dressings

is smaller. Further, it has no poisonous properties, and eczema was never observed in its use. (H. Ranke, Lancet, June 22, p. 916.)

DISLOCATION OF THE HIP.—As preliminary to reduction, the patient should be etherised to relaxation, and, in order to give the surgeon control of the limb, laid on the floor. It is well to remember that the head of the femur always faces the same way as the internal condyle. If there is any single and best rule for reducing a recent dislocation of the hip, it is to get the head of the femur directly below the socket by flexing the thigh at about a right angle, and then to lift or jerk it forcibly up into its place, This rule applies to all dislocations except the pubic, and even to that when secondary from below the socket. If, after one or two trials, it should appear that the hip cannot be jerked into place, let the rent in the capsule be enlarged a little by moving the flexed thigh, not up and down, but from one side to the other, so as to sweep the head of the femur across below the socket. No danger need be apprehended from this expedient of circumduction. The added injury is a very slight one. So long as air is not admitted to the wounded parts the lesion is no more serious than often occurs in a simple fracture of the thigh. The following are rules for reduction in the *dorsal* dislocation—1. *Flex and forcibly lift.* If this fails,—2. *Flex and lift while abducting.* If this fails, it will be found that abduction has carried the head of the bone from the dorsum nearly or quite to the thyroid foramen, and that the capsular rent has been so enlarged that the first method may now prove successful. Lifting the femur abducts it if it raises the pelvis on the lifted side. In *thyroid* dislocation, *abduction* of the flexed thigh reverses this movement and carries the head from the thyroid foramen to the dorsum, also enlarging the opening and making the first rule effective. The *pubic* dislocations may be generally brought down, after flexion, without difficulty from above the socket. If they are secondary, the head of the bone will fall after flexion, to its previous position below the socket, and may be reduced from there like the thyroid. (Dr. H. J. Bigelow, p. 159.)

Nothing can be more simple than the reduction of secondary dislocations from below the socket. If dorsal, and the flexed thigh has fallen to a horizontal position by a spiral movement downward and inward, reverse the movement and reduce the limb by a spiral upward and outward. If thyroid or pubic, and the knee has followed a spiral downward and outward, reduce it by a similar movement upward and inward. But the spirals of reduction practically amount to mere flexion; therefore, in either case, flex the limb and jerk it

upward; and in the thyroid luxations it is well to aid the process by outward traction with a towel or by a fulcrum in the groin. Finally, if the capsule offer resistance, sweep the flexed femur from side to side to separate its fibres and get the head below the socket, before jerking the bone up into place. (Dr. H. J. Bigelow, p. 163.)

DRAINAGE BY CATGUT.—I beg to recommend as worthy of the notice of the profession the principle of drainage by utilising capillary forces through skeins of an absorbable material like catgut, if by its use the evils of the drainage-tube, already referred to, are got rid of. We may now anticipate a time when, with catgut stitches instead of silk, horsehair, or silver wire, catgut drains instead of india-rubber tubing, and chronic-acid gut fixing together the buttons instead of silver wire, it will not be necessary to uncover our wounds from first to last during healing, when the deep dressing need never be shifted, and when the outer dressing will only require to be removed when soaked with discharge. The amount of discharge, in its turn, will be reduced to a minimum by the use of an absorbable animal material like catgut, instead of a non-absorbable foreign body like silk, silver wire, or india-rubber. Chromic-acid catgut sutures are of great value in a very large class of cases, and may be regarded as perfectly reliable substitutes for silk or wire, as, with the single exception of absorbability, they will be found to meet all the requirements of this mode of keeping cut parts in apposition. For chromic-acid catgut possesses the advantage of a maximum degree of strength combined with a minimum of size, and it is absolutely non-irritating. The mode of preparation I have hitherto found most reliable is this: I take a few yards of thin, common, unprepared, dry catgut, and place it in a wide-mouthed phial containing glycerine and carbolic acid (one part of acid to seven of glycerine), and, after steeping for a week or longer, the catgut is transferred to a vessel containing chromic acid, acetic acid, and water (one per cent. of chromic, and twenty-five per cent. of acetic acid.) At the end of seven hours the catgut is taken out and dried. During the drying process it is prevented from curling up, or getting rough and uneven, by using tension, the gut being stretched by winding it round two nails driven into a piece of wood, a foot or two distant from each other. In the course of a few hours it will be found ready for use. Catgut, thus prepared, is a brownish black, perfectly smooth material, possessing great strength, yet not much thicker than what is sold in bottles as "fine" antiseptic catgut ligature. (Mr. Chiene, Dr. O. Will, p. 179.)

DUPUYTREN'S FINGER CONTRACTION.—The operation and treatment of this form of finger contraction by the subcutaneous division of the palmar fascia and immediate extension, which I have found successful in practice, may be described as follows:—1. The subcutaneous division of all the contracted bands of fascia which can be felt; the bands to be divided by several punctures, with the smallest tenotomy-knife passed under the skin and cutting from above downwards: a pledget of lint being at once placed over each puncture and retained in position by a strip of plaster. 2. Immediate extension to the full extent required for the complete straightening of the fingers, where this is possible, and the application of a retentive well-padded metal splint from the wrist along the palm of the hand and the fingers; the fingers and hand to be bandaged to the splint. 3. The bandage not to be removed until the fourth day, when the lint and plaster may also be taken off, as the cutaneous punctures are always found to be healed by the fourth day. The retentive metal splint to be reapplied, and the hand and fingers bandaged to it. 4. Extension to be kept up by the splint worn continuously night and day for two or three weeks; but the splint and bandage to be changed every two or three days. After this, the extension splint is to be worn at night only, for an additional three or four weeks, free motion being encouraged during the day. (Mr. W. Adams, p. 167.)

FRACTURE, UNUNITED.—In a most obstinate case Dr. Patterson inserted a piece of dog's bone. He says "on the 14th of September the patient was taken into the theatre and placed under the influence of chloroform, while at the same time a retriever dog was being anæsthetised. I made an incision along the ulnar side of the arm, cutting down upon the ends of the fractured bone, and removing the fibrous band which alone formed the bond of union; the rounded points were removed by the saw, and a hole drilled obliquely through each squared end. The same process was repeated on the radial side, when it was found that an interspace of about three-quarters of an inch existed between the two fragments of the radius. In the meantime, Mr. Andrews, one of the senior students, and a very clever manipulator, had exposed the humerus of the quadruped, completely denuded of every tissue except the periosteum. The length of bone was accurately measured (three-quarters of an inch), while from half an inch beyond the end of the necessary length the periosteal covering was rapidly but carefully dissected, the bone sawn through, a hole drilled in either end obliquely, as in the radius and ulna, and at once placed between the ends

of the radius, where it fitted accurately. Wires having been passed through the holes, the bones were firmly tied together, the loose half-inch margin of the periosteum of the foreign bone being carefully spread over the periosteum of the radius. The wound was stitched with silver wire, the bone sutures coming out at each end of the incision. Wires were passed through the ulna, tied together, and the wound treated in a similar manner. The entire operation was conducted under the carbolic acid spray. The arm was put up in gauze, and held in two rectangular splints. On Nov. 3rd, the ulna was found to be firmly united; but on the radial side small pouting granulations appeared, as if a foreign body were present, which, however, could not be detected by the probe. On Nov. 28, the patient was put under chloroform, and the wires removed." (Dr. A. Patterson, p. 148.)

FRACTURES OF THE LEG.—*Plaster-of-Paris Splints.*—For more than two years I have been using at this hospital plaster-of-paris splints for all simple and for most compound fractures of the lower extremity. The apparatus consists of first, inside and outside splints made of common house-flannel and plaster-of-paris; and, secondly, of muslin bandages. The splints for fractures below the knee are shaped somewhat like the old short outside splint. The foot-piece is, however, wider. The splint for the inside of the leg is similar in length and width to that for the outside. The splints should be long enough to extend from the tubercle of the tibia to the middle of the metatarsus, and together they should be in width about one inch less than the circumference of the limb at the corresponding part. A rough guide to the shape of the splint may be found in the injured person's stocking when it is laid flat on a table. Each splint is constructed of two layers of the flannel. The outer layer carries the gypsum. The inner layer forms a dry, warm, elastic lining, and protects the skin. These splints are applied by means of the muslin bandages. The bandage is put on like any other, from the toes to the knee. One thickness is enough. Two bandages of five or six yards in length are more convenient than one of ten or twelve yards. *To make the splints.*—1. A piece of house-flannel or an old thin shrunk blanket, or any suitable substitute is selected. The pieces may be shaped by measurement, taking the circumference of the limb below the knee, at the biggest part of the calf, just above the ankle-joint, from the front of the ankle-joint round the heel to the front again, and at the middle of the metatarsus. The flannel of each splint should be in width half an inch less than half the circumference at any of those points. The width of the two

splints should be one inch less than the circumference of the limb at any corresponding part. It should be long enough to extend from the tubercle of the tibia to the middle of the metatarsus. Four pieces are required—two for each splint. 2. Two bandages of common muslin are prepared, each five to six yards long and two inches and a half in width. 3. About a handful of good dry plaster is mixed with water to the consistence of thick cream. 4. The inside pieces of flannel may be laid on the table or bed, the outer surface being upwards. 5. The outside pieces are to be soaked in the plaster separately, and laid out on their respective inside pieces. *Application.*—Whilst traction is kept up, and the ends of the broken bones are maintained in apposition, the splints are to be applied and smoothed; then the bandage is to be put on. Traction is to be maintained during the hardening of the plaster. The latter takes place in about three minutes. (Mr. J. Croft, p. 154.)

PARAFFIN SPLINTS.—Paraffin, as a material for the formation of splints, possesses the following advantages:—1. It is always ready for use, and may be kept for any length of time without spoiling or undergoing change, unless it is exposed to heat. 2. It is easy of application, requiring no special training or dexterity, and it may be ready for the surgeon about fifteen minutes after he has ordered it. 3. It sets readily, and yet not too soon. Starch and glue take a much longer time to harden, glass longer still, while plaster-of-Paris sets too quickly for many purposes. 4. Before it sets it may be cut with a pair of scissors and afterwards with a sharp knife. 5. It may be made into a splint quite impervious to discharge or moisture of any kind. 6. It is light, easy, and comfortable to the patient. 7. It may be formed into a semi-elastic bandage, or a firm solid splint. 8. It may be used many times. After one splint is finished the paraffin can be remelted and reapplied. (Dr. W. Macewen, p. 153.)

STIFF FINGER.—*Resection of the Phalangeal Joint.*—A stiff finger to a labouring man, especially when engaged in certain occupations, often means inability to earn his livelihood. Resection of the finger-joint is as applicable and as useful in proportion as resection of the knee-joint, and yet how often does one see what might be made a good and useful finger amputated for disease of one of the phalangeal joints, simply, I think, because attention has not been called to the preferable way of treating them by excision, and it does not occur to those who amputate. It may be considered a very trival matter, but every detail in surgery has its importance, and cannot

afford to be neglected; and to those whose fingers are the means of their livelihood the question of amputation or excision is to them often a matter of pounds, shillings, and pence. (Mr. E. Prideaux, p. 146.)

WOUND OF THE SUPERFICIAL PALMAR ARCH.—In a case of wound of the superficial palmar arch, I bound the hand to a dorsal splint, after plugging the wound. He soon returned to me bleeding as profusely as before. I then determined on acupressure, and taking a stout harelip-pin, passed it through the tissues about half an inch from the edge of the cut, under the artery, and out again to a corresponding distance the other side of the wound, and placed the limb again on the splint. This had the effect of entirely stopping the bleeding; the needle was removed on the fourth day, and the entire wound had closed by the end of the week. (Mr. E. Bellamy, p. 182.)

AFFECTIONS OF THE SKIN, &c.

ACNE.—If at the onset of the affection the sebaceous glands and follicles become affected and overloaded, they should be relieved by pressure between the finger and thumb-nail or a large watch-key, and by frequent washings with warm water and oatmeal, after which a good rubbing with a fleshbrush will remove the contents of a number of the pimples. If the disease is more advanced, and the indurated spots become enlarged and painful, the vapour douche is then of much service, followed by an ointment of the ammonio-chloride of mercury (thirty grains to the ounce, and camphor one ounce), or the gentle application of iodide-of-sulphur ointment at night, and the use of a cooling zinc or calamine lotion and glycerine (of the former about two drachms of each to the ounce), to be painted over the part affected two or three times a day. With the judicious use of the above remedies and applications we may probably remove and cure the disease; but troublesome redness, with often great vascularity of surface, still remain in spite of all treatment, and it is to this stage of the affection these remarks tend. To these small indurations and superficial venous enlargements of texture I apply the strong acid nitrate of mercury with care by means of a spun-glass brush. One or two applications, with the immediate use of blotting paper, will usually suffice to effect complete obliteration of the veins without a scar, and I find after a few days the morbid state of the capillaries has undergone a complete change for the better. Nevertheless, we may still have some of the larger capillaries, which keep

up the localised redness and induration, to contend against; then the mercurial acid treatment does not hold good. I therefore divide each engorged superficial and conspicuous vessel with the point of a lancet, and, should the hemorrhage be much, I apply a small ring of silver or steel, about the eighth of an inch in diameter, which I have fixed into a handle at right angles, over the divided ends of the vessel. I then insert a minute grain of nitrate of silver upon the end of a probe, which at once stops the bleeding, and soon obliterates the engorged vein, leaving only a small, black, discoloured spot, which may easily be removed by the application of a solution of iodide of potassium. (Mr. J. Startin, p. 230.)

Caustics.—*The Ethylates of Sodium and Potassium.*—The change of ethylic alcohol into sodium alcohol transforms it from an irritant to a caustic. Laid on dry parts of the body the sodium alcohol is comparatively inert, creating no more change than the redness and tingling caused by common alcohol; but so soon as the part to which the substance is applied gives up a little water, the transformation I have described above occurs; caustic soda is produced in contact with the skin as water is eliminated by the skin, and there proceeds a gradual destruction of tissue, which may be so moderated as hardly to be perceptible, or may be so intensified as to act almost like a cutting instrument. I do not as yet see the means of applying readily these two active alcohols for internal administration, but I can predict for them a very extensive application for external purposes. They are most potent caustics. In some cases they may be employed to destroy rapidly such morbid growths as are not favourable for excision by the knife. In many cases of cancer they will prove invaluable, and will, I believe, exert a direct local curative influence. Injected into morbid growths, they would so quickly destroy them that the action might have to be conducted while the body was under the influence of an anæsthetic. In being applied direct to the sensitive unbroken skin, I find that their destructive action is less painful than would be expected. I have made with both compounds a superficial eschar on my arm with no more pain than a slight tingling warmth. What is more, when pain is felt, it may be checked quickly by dropping upon the part a drop of chloroform, which decomposes the alcohol, converting it into a chloride salt, and an ether—triethylic—which is inert locally. Again, I find that these alcohols dissolve some of the vegetable alkaloids. Thus opium may be dissolved in them, and a solution of opium in caustic alcohol is made directly by mere addition of the nar-

cotic to the caustic spirit. Practical men will see the advantages of combinations of these alcohols with narcotics. The practice opens the way to one of the greatest needs of medicine—a sure, rapid, and painless caustic. The caustic alcohols may be used in combination with local anæsthesia from cold. A part rendered quite dead to pain, by freezing with ether spray, could be directly destroyed by the action of caustic alcohol—a practice very important in the treatment of poisoned wounds, such as the wound from the bite of a snake or a rabid dog. It is by no means improbable that some cystic tumours may be cured by the simple subcutaneous injection of a little of these fluids after destruction of sensibility by cold. (Dr. Benj. W. Richardson, p. 351.)

LEPROSY.—*Chaulmúgra Oil and Tincture of Psoralea.*—In the treatment of six cases of leprosy the two drugs employed were the chaulmúgra oil and seeds, and a preparation of the seeds of psoralea corylifolia. The chaulmúgra oil usually found in the bazaars being very impure, supplies of the purest oil were obtained from Calcutta. A tincture of psoralea was prepared by macerating ten drachms of the seeds in a pint of rectified spirit, and a liniment by adding a drachm of the tincture to an ounce of chaulmúgra oil. The doses varied in different cases from five to twenty drops three times a day. The oil should be given at the outset in small doses and gradually increased, as otherwise it is apt to cause nausea. The good results were seen earlier in those cases in which the powdered seeds were given instead of the oil. A liberal milk diet seemed to be a valuable auxiliary. The psoralea given internally, in combination with the oil, appeared to be of no value, but when applied externally in the form of a liniment was of undoubted service, especially in stimulating the hair bulbs. (Dr. David Young, Florence, p. 356.)

PSORIASIS.—Dr. Squire recommends that chrysophanic acid in the treatment of psoriasis should be used in the form of ointment, for which the following formula gives such a full strength preparation as I have accustomed myself to employ: ℞. Chrysophanic acid ʒ ij., lard ʒ j. Digest the acid in the lard at the temperature of boiling water for half an hour, stirring constantly. When "set," mix with pestle and mortar. This strength of ointment will be found generally suitable, but it may, of course, be varied, and reduced in strength to suit cases where the skin is extremely sensitive. To prevent disagreeable results from over-action of the remedy, as well as to ensure that it is not employed in a careless or half-hearted manner, I recommend that personal

attention of the medical man should be given during the first week of treatment, and that the case should be seen from time to time while the treatment is continued. The details, if carefully supervised in the early stage, will ensure a progress within a few days that otherwise will not be made in several weeks. These details, together with the accidents of treatment and the *modus operandi*, of the remedy, will be best understood by portraying a typical case. The patches of psoriasis are first rubbed well with a piece of stout blotting-paper or common rag, moistened with benzole, the object of which is to absolve and remove that natural greasiness of the skin which, like a film, opposes the ready penetration of the skin by the specific. I believe that benzole—a tar product—has also some specific action, and is therefore otherwise a useful adjunct. This preliminary application of benzole facilitates the next application, viz., hot water freely used with soap, the object being that of softening the layers and fragments of epidermic scales. The removal of the scales is a matter of prime importance, and must be effected whether by scraping with a blunt knife, or by scratching and picking with the finger-nails. It is only when the film of epidermic scales are removed that the skin proper can be fairly attacked, and I have not been deterred by the starting of a little blood from pushing to an extreme degree this scraping and scratching operation. The ointment is now rubbed in very perseveringly for several minutes at a time with the tips of the fingers. As the ointment is a dye, and will stain for some days the fingers and nails, I occasionally prevent that disagreeable accident by recommending the use of indiarubber finger-stools such as are frequently employed by photographers. These three stages of local treatment, viz., the wiping with benzole to remove grease, the thorough soaking of thickened patches of skin with hot fomentations, washing with soap, diligent scraping away of epidermic *debris*, and the patient rubbing in of the ointment, may be employed three times a day if the patient can command the necessary time and facilities. There is no question that the more frequent the application the more rapidly does the disease come under the action of the curative agent. But under ordinary circumstances a morning and evening application seem to give a sufficiently satisfactory result. Dr. Adams has tried a watery paste of alizarin with apparent success, the alizarin made up in the strength of ʒ ij. to the ounce of lard, and used precisely under the same conditions as the chrysophanic-acid ointment. There is an analogy in the properties of alizarin and chrysophanic acid. So far they seem to be therapeutically isomeric, and my mind has been all the more

inclined to admit the resemblance, because I have ascertained that they are chemically isomeric. I anticipate as possible and probable that alizarin may either prove a cheap substitute for chrysophanic acid, or that it may form a basis or a link in the artificial production of chrysophanic acid at a cheap cost. (Dr. J. Adams, p. 219.)

RODENT ULCER.—Dr. Squire treats these cases by scraping or cutting out the diseased portion by means of a sharp spoon. He says, I operated (chilling the skin first by the ether spray) by means of one of my own sharp spoons, carefully scraping away all the friable tissue, including the raised beaded margin. The process was effected without pain, and even without sensation of any kind except the sensation of cold occasioned by the ether spray. No blood, not even a drop, was lost. Fourteen days after the operation the wound was completely healed, and the patient consequently cured. Up to the present time no re-appearance of the disease has yet occurred. The floor of the rodent ulcer is extremely friable and rotten in texture, and so equally is the thin, and to the touch seemingly hard, margin of the ulcer. The friable texture comes away like soft putty under the touch of the spoon, while the tough subcutaneous fibrous tissue, as well as the equally fibrous tissue of the surrounding sound skin, is quite impervious to the touch of the spoon. In this way, by exercising only moderately firm pressure with the spoon, the sense of touch greatly supplements the eye in differentiating the diseased from the sound structure. (Dr. B. Squire, p. 360.)

SMALL-POX.—*Carbolic Acid Ointment.*—The exact proportion of ingredients in the ointment used for small-pox patients is as follows:—Carbolic acid, twenty minims to half a drachm; glycerine, a drachm and a half; oxide of zinc ointment, six drachms. This was freely painted over the face and hands every day, or every other day, with a large camel-hair brush. On the patient convalescing, the dried ointment and scabs came off like a plaster of mould from the face. The skin was tender and marked with red spots, which gradually died away; but there was no pitting. At Edinburgh about fifteen years ago several kinds of preparations were used to prevent pitting. Collodion, solution of caoutchouc, zinc ointment, &c., were thus employed. Owing to their drying and cracking, they all required to be repeatedly applied, so that the atmosphere, or, as we have since learned, the germs in the atmosphere, were prevented from coming in contact with the pustules. In the words of Professor Tyndall, the pustules were thereby "prevented from becoming a nidus for

the propagation of septic bacteria." Collodion and the like act mechanically, whereas carbolic acid acts as a direct antiseptic; and the glycerine and ointment, besides being a convenient vehicle for the acid, mechanically prevent the passage of air-germs. The ointment is useful for many other purposes, which will suggest themselves to the mind of the practitioner. For instance, when the pustules of vaccinia take on an erysipelatous action, the free application of the ointment soon reduces the inflammation. (Mr. Thorburn Paterson, Lancet, Aug. 24, p. 275.)

ULCERS AND VARICOSE VEINS.—Having for some time used the elastic bandage as recommended by Dr. Martin, of Boston, U.S., for the treatment of varicose veins and ulcers, it is right to express my opinion that his treatment is practically good. Under its influence, ulcers not of a specific character do certainly heal, and quickly; and as this is effected whilst the patient follows his usual occupation, the merit of the treatment is evident. The bandsge should be applied before the patient rises from his bed in the morning—before, that is, the veins of the leg become distended by the impeded column of blood within them. It should be applied with just snugness enough not to slip down. The moment the foot is put to the ground the limb is so increased in bulk by the increase of blood in the veins that the bandage becomes of precisely the proper degree of tightness, and, no matter how active the exercise or labour of the patient, it will remain in position all day. When the patient undresses at night the bandage is to be removed, and the limb wiped dry, a piece of soft old linen moistened with oil, or some equally simple dressing, laid on the ulcer and retained in place by a few turns of an ordinary roller. The bandage should be sponged with water (cold will do, but warm is better), and hung over a line to dry in readiness for the morning, or it can be wiped dry at once, or rolled up with the tapes in the centre. Such is the dressing for the night; in the morning the leg can be washed, but, whether it is or not, all traces of oil or cerate should be carefully wiped away, as contact with the bandage of any fatty matter would tend gradually to injure the rubber. This is the whole treatment. Rubber bandage all day, with erect position and exercise. The simplest possible dressing (merely to protect the ulcer from injury), with the horizontal position and rest, all night. Any pimples which may form are left to be treated by the rubber, and if the skin become chafed a light covering, as of bunting, is applied as a bandage under the rubber, and the same is recommended to absorb excessive moisture. (Mr. G. W. Callender, p. 225.)

VARICOSE ULCER.—In a case of varicose ulcer, on lifting the foot above the level of the trunk the vein is immediately emptied of its blood, and it fills again on its being placed firmly on the ground. It *fills, however, from above, and not from below.* During the circulation in its normal state the valves do not, as alleged, contribute to maintain the onward current by supporting the columns of blood in advance of them; the cusps during this process are flattened against the sides of the veins, and are only subjected to closure, and that temporarily, for the purpose of adjusting the blood-tension throughout the veins constantly in peril of disturbance. Aided by certain plexiform arrangements, *this* is the main purpose of the valve system. It can only, therefore, be when a vein is diseased to the extent that it is simply passive in relation to its current, as attested by the behaviour of the blood on alternately raising and depressing the limb, and, at the same time, its *valve—always a barrier valve*—is rendered incompetent, that its part in the composite function of regulating blood-tension is suspended. The result is that in this especial vein, the weight of the superincumbent blood-column is in excess of the forces engaged in propelling it; the skin bordering and intermediate between it and the valve barrier next below becomes congested, and ultimately ulcerates. The resultant sore is a genuine "varicose ulcer." If the tissues be relieved of their superfluous blood by rest and leg elevation, the sore will heal; but it will repeat on adopting an opposite course. The only effective remedy consists in the establishment of a quasi-valve in that part of the vein in which it has become inert; and this can only be done by some one of the ordinary means of effecting its obliteration. Thus the essential factors in respect of a *varicose* ulcer are extreme vein degeneration, and resultant incompetency of its valve—always a *barrier* valve; the cure, its substitution by obliteration of the vein-channel in that situation. (Mr. John Gay, p. 228.)

WOUNDS.—The following is an illustration of the mode of treating wounds at St. Bartholomew's Hospital. The case was one of in-knee, and the knee-joint was opened and the internal condyle separated from the femur. No carbolic or other spray was used at the time of the operation or at any time subsequently. The wound was covered with a doubly folded piece of lint soaked in carbolised oil (1 in 12); over this was placed a larger piece of lint, likewise soaked in the oil; and over all a yet larger surface was covered with some ordinary gutta-percha tisue. The dressings were examined daily, the gutta-percha tissue being lifted and the outer layer of lint being changed, the gutta-percha being

then replaced. The inner layer of lint was left undisturbed on the wound for three or four days, being each day refreshed with carbolised oil (poured upon it or painted over it with a camel-hair brush). Whenever this layer of lint was changed, the wound was freely exposed to the air. The patient got well without any constitutional disturbance, and the parts healed in the ordinary course. Treated in a similar manner, there are at this time under observation in one of my wards a severe compound fracture of the femur into the knee-joint, and two cases of compound fracture of the leg; in one of the latter, it was not necessary to disturb the inner layer of lint for nine days after its first application. The results claimed for antiseptic surgery can be ensured by the plan which we have now used for many years, and of which I venture to write as being as simple as it is efficacious. Whilst other methods aim, amongst other things, at the entire exclusion of germs from wounds, I shall rest content to allow their ingress, feeling quite sure that after such entry they are absolutely prevented from doing any harm by the treatment we adopt. (Mr. G. W. Callender, p. 231.)

SYPHILITIC AFFECTIONS.

MERCURY IN SYPHILIS.—*Modes of Administering.*—There can be no doubt that the majority of cases in private practice, when they require mercury, are best treated by the oral method. If small doses be given in the first place, and gradually increased in frequency till the desired effect be produced, the chances of gastric or intestinal irritation are easily avoided, and salivation can be prevented by the use of chlorate of potash, or alum wash after each dose. The patient is thus permitted to follow his usual avocation, and the remedy may be continued with proper precautions, as long as may be needful. With the other methods it is difficult, from their inconvenience, uncleanliness, or disagreeableness, to induce the patient to persevere sufficiently long; and one strong objection to the routine use either of mercurial fumigation as practised by Mr. Lee, or inunction as practised by Sigmund, lies in the necessity for confinement to the house, and even for a considerable portion of the day to the recumbent position. Exercise in the open air, a nourishing dietary, and even a certain amount of stimulation, especially in chronic cases, are of great importance, whatever be the way in which the mercury be given. Free action of the skin is very desirable, and the occasional use of the vapour bath is a good method of securing it. In obstinate cases of skin affection, an occasional mercurial vapour bath may advantageously be added,

and in hospital cases mercurial fumigation is probably the most efficient plan, at all events, to begin with. (Dr. J. Duncan, p. 239.)

MIDWIFERY, ETC.

METRORRHAGIA.—*Hypodermic Injection of Ergotine.*—M. C. Paul, in Bull. de Therap. gives the details of 14 cases in which the uterine hemorrhage was arrested in from 5 to 16 minutes, by the following solution:—Ergotine, 2 grammes; water, glycerin, āā, 15 grammes; of which from one to two grammes were injected. M. Paul concludes that the hypodermic injection of ergotine is the most rapid and the most efficacious means that we have at our disposal in the treatment of metrorrhagia. (Med. Press and Circular, March 20, p. 245)

OVARIAN TUMOURS.—*Diagnosis between Ovarian Dropsy and Ascites.*—We have a patient who has a collection of fluid, either free in the abdominal cavity, or contained in some cyst. In the first case, the central part is dull on percussion, and the sides are quite clear. The inference would be at once that, with intestines at the sides and the fluid in the centre, the woman lying on her back, the fluid must be enclosed in a cyst. On the other hand, here the intestines are floating, giving a clear sound on percussion, and the fluid gravitates to the most depending parts of the cavity, and you will see at once the fluid must be free; the intestines are floating about and the fluid is beneath. As a rule, I suppose, in nine cases out of ten, that would probably be the correct diagnosis. You would say here was a case of ascites, and here, where the intestines are pressed backwards and the fluid is in the centre, that will be encysted fluid; and, in a large number of cases, that will be certainly true. But there will be exceptions. There may be so much fluid in the peritoneal cavity that the intestines do not touch the front of the abdomen, or the intestines may be fastened backwards by adhesions; and fluid, which appears to be in front and is in front, is there, not because it is enclosed in a cyst, but because the intestines are held back. And, on the contrary, one may get a case where fluid is enclosed in the front by coils of intestine round it, and is circumscribed in that manner. The alteration of the position of the patient makes considerably greater and more rapid changes in cases of ascites than in patients with ovarian cysts; the cyst moves about much more slowly than fluid. The superficial veins are more commonly enlarged in ascites than in ovarian dropsy, and, on measurement, the enlargement of the abdomen in ascites is more likely to be

symmetrical than in the ovarian cyst; and then, on feeling the abdomen, you are more likely to get a flaccid abdominal wall in ascites, and a more tense wall in a cyst; and very often, in ascites, the greatest girth is at the umbilical level; whereas, in ovarian dropsy, it is sometimes considerably lower. Supposing doubt does arise as to whether fluid is free or not, one has always the resource of tapping; and a chemical and microscopical examination of the fluid that is removed will settle any doubt as to whether it is free peritoneal or ovarian fluid, or fluid of some other cyst. Years ago, before Scherer's test to distinguish albumen in its true form from the forms of albumen that chemists do not seem very accurately to determine, it was sometimes extremely doubtful to arrive easily at this determination. But Scherer showed that the albumen which, when coagulated by heat, is redissolved by acetic acid is a very different albumen from true albumen. Paralbumen or metalbumen are forms that, when coagulated by heat, can be dissolved by acetic acid boiled. One takes a test-tube and boils the fluid, and the albumen is coagulated. You add a little strong acetic acid, and give it a good boil, and shake it up; and if the albumen be true, the coagulum does not redissolve in the acetic acid. But supposing it to be paralbumen then it either dissolves, or forms a whitish transparent fluid, or breaks up into a kind of jelly-like transparent mass, which is quite easily distinguishable from undissolved albumen coagulated by heat. This was supposed to be, for a long time, a pretty certain means of distinguishing between ascitic fluid and ovarian fluid. If the coagulated albumen were dissolved in acetic acid, it was said to be ovarian, and if it did not redissolve it was said to be ascitic; and that was frequently right. Sometimes part would redissolve and part would not, and then the supposition was that it was a mixed fluid, some ovarian and some peritoneal, that an ovarian cyst had burst, and some of the fluid was in the peritoneal cavity; a certain amount of fluid had been poured out, and there was a mixed fluid, which contained some true albumen and some paralbumen. After a little further examination, one saw that there were exceptions to this rule. (Mr. T. Spencer Wells, p. 287.)

Diagnosis of the Different Kinds of Ovarian Tumour.—We can ascertain with little care whether a cyst is a single cyst with limpid contents or whether the contents are likely to be viscid, and this will assist us in forming some opinion as to whether one should be content with tapping and drainage, or whether it would probably be ultimately necessary to resort to ovariotomy. The reasons which would induce us to suppose that the cyst is single, and that it probably may be

cured by tapping, are, that it may have existed for many years without any great effect upon the general health of the patient; or, on the other hand, that it has formed with such extreme rapidity that it is almost certainly mistaken for ascites. When you get this very rapid formation of a cyst, it is pretty certain the cyst is a single one; and so also if the cyst has existed a good many years, and the patient has not suffered much from it, it is probably single. And these are cases in which tapping alone may not only afford temporary relief to the patient, as it does, but may possibly complete the cure; the fluid may never re-form. When you get resistance to the wave of fluctuation in different directions, then you may also be pretty certain that there are septa dividing the cyst into different portions, and that it is of a multilocular character, and in that case tapping would be of no use; although one compartment might be emptied, the others would remain full. The solid tumours I need not say anything more about, except that they do not fluctuate; they are solid. These are much rarer than cases where one finds a portion of the tumour solid and a portion of it fluctuating. (Mr. T. Spencer Wells, p. 290.)

Tapping an Ovarian Cyst.—Mr. Wells uses for tapping an instrument which he describes as follows:—The point is exactly like the point of a steel pen, and is so made that it does not cut a piece out of the abdominal wall, but simply enlarges the opening made by the scalpel. A small opening is made first by a lancet or scalpel, and then this instrument is easily passed into the cyst. It is evident, if this were left in the cyst, it would be dangerous when the cyst was empty; a sharp point like this in the interior of a contracting cyst would be pretty certain to wound it; so I added a blunt cannula. I first put the cutting point inside, but afterwards found it better to slip up the blunt cannula inside the sharp tube, and that is the form of instrument now used. Passing it with the point directed downwards so as to avoid any rush upwards of air which might occur if the point were at a higher level than the end of the tube. With the tube, the instrument at once becomes a syphon, and this calibre is large enough to allow the escape of viscid fluid. There are several small openings in the cannula, because if the opening be too large, there is the possibility of the entrance of little bits of omentum or small shreds or clots, which may be in the interior of the cyst. These would block the tube; therefore it does not do to make it too large. Fixing a long elastic tube on to the end of this instrument, the fluid is carried silently away into a vessel under the bed; the patient does not hear any splashing,

and there is no possibility of any air entering into the cyst. Supposing the fluid—whether ovarian or cystic—is in the peritoneal cavity, or there is a doubt as to where it is, it is desirable to remove such fluid without the possibility of wounding any uterine tumour, or any mass of cancer, or injuring the intestines or omentum. For this operation I contrived this form of cannula, which is used after a simple lancet puncture. Passing a lancet or small scalpel through the abdominal wall until fluid appears, then removing the knife, and pushing this hollow tube to the cavity, the fluid at once rushes away. The tube may be moved about as a probe to feel whether any tumour is free or adhering, and we can do that without the slightest fear of doing any harm. With these several small openings, there is no fear of portions of omentum becoming entangled by the pretty strong suction that goes on through the long syphon. (Mr. T. Spencer Wells, p. 294.)

OVARIOTOMY.—*The Adhesions.*—Practically, I think, the question of extent of adhesion to the abdominal wall is not of much consequence. The result is pretty much the same, so far as the adhesions go, to the patient; but the amount and intimacy of pelvic adhesions are a matter of very much greater moment. Supposing an ovarian cyst is adherent low down in the pelvis, between the uterus and rectum or the uterus and bladder, or on either side, the attempt to separate it is necessarily a dangerous one. If any bloodvessels be torn, it is difficult to find them. One can pretty well ascertain if there be such adhesions as these, by a careful examination of the pelvis by the vagina. You find that they do not move with the position of the patient; they do no move when she coughs; they do not move when the shoulders are lowered and the hips raised; and it is impossible, either with the uterine sound or in any other way, to separate the uterus from these pelvic portions of the adherent tumours. Occasionally there will be an ovarian tumour low down in the pelvis which does move, from which the uterus can be separated, and need not interfere at all with ovariotomy; and in such cases it is sometimes quite curious to hear the rush of air into the bottom of the pelvis when the lower portion of the cyst is separated from the hollow of the sacrum. Air rushes down with a gurgle, the tumour is easily brought up, and there is no more difficulty than in an ordinary case. It occasionally becomes very doubtful whether an ovarian tumour is simply jammed down into the pelvis, or whether it is fixed there by strong adhesion; but still I think this can be made out by care—ascertaining whether the uterus is movable; ascertaining

whether the lower portion of the cyst moves in different positions of the patient; and, if it really be firmly fixed, whether the sensation to the finger is that it has been absolutely glued down to the bottom of the pelvis. If there be this intimate adhesion pretty low down, as a rule I think the operation is not one that is likely to succeed. (Mr. T. Spencer Wells, p. 292.)

Some Points in Ovariotomy.—If the pedicle be small enough to be securely held in a clamp of moderate size, and long enough to permit the clamp being fixed outside the closed wound, without much pull on the uterus or broad ligament, no more ready or more successful method than the clamp can be desired. When the pedicle is so short, broad, or thick that a very large clamp would be necessary to secure it, that the wound could not be closed around it, and the traction upon the uterus when the clamp was fixed outside the abdominal wall excessive, either the ligature or the cautery should be preferred to the clamp. Or if, after applying a clamp, it be found that it cannot be fixed outside without undue strain upon the uterus, either the cautery may be used, or the pedicle may be transfixed and tied, and the clamp removed after it has served its temporary purpose. As a rule, if adhesions be torn and the tumour removed, the abdominal wall contracts, and this contraction stops the bleeding; but if the adhesions be cut by scissors or knife, they do sometimes bleed, and in a troublesome way. For cases in which there is hyperpyrexia, Mr. Thornton has contrived a form of cap which answers the purpose extremely well. A pail of water, with a large lump of ice in it, is placed by the bedside of the patient, and the stream of iced-water runs through the cap, which is formed of a series of tubes of indiarubber, lined with linen. This is placed upon the patient's head. It is made of different sizes to fit different shaped and sized heads; and this other tube is put into a second pail on the other side of the bed, and by this means the head is iced. The effect in lowering temperaturp is very marked. It is very seldom that within an hour the effect upon the patient cannot be distinctly proved by the thermometer. (Mr. T. Spencer Wells, p. 302.)

PLACENTA PRÆVIA.—If the os uteri is small and rigid, our duty will be, in the first place, to have recourse to plugging, until this state of the os is overcome; and the best kind of plug is the indiarubber bag filled with air, which Dr. Keiller had the merit of introducing into midwifery practice. This is infinitely superior to "Dr. Barnes's bags," as they are called, which are filled with water. The bag filled with air not only affords a light and good support, but it enables the accoucheur

to ascertain if the hemorrhage is still going on, and it is easily applied; whereas, if a sponge or handkerchief is employed, it is introduced with difficulty, and the blood is prevented escaping, so that the accoucheur is kept in the dark as to the continuance of the hemorrhage, unless the general condition of the patient enlightens him. If the labour pains are active, it will be desirable to remove the plug to ascertain what progress has been made in the dilatation of the os, and if it is sufficiently dilated, or easily dilatable to admit the hand, and the child has been ascertained to be alive, and the hemorrhage profuse, there ought to be no delay in delivery by means of turning. But if the child is dead, and the mother much exhausted, it may become a question if the entire separation of the placenta may not be attempted, especially if there is a natural tendency to its being detached by the uterine contractions. If the os uteri is not sufficiently dilated to admit of either of these operations, and if the case is one of central presentation, the plug should be again employed, as it is probable that the hemorrhage is caused by the placenta being put on the stretch by the pressure of the child's head, and the support afforded by the plug may have the effect of checking it until labour is further advanced. But if it is a partial presentation, and the distended membranes are found occupying the entire disk of the os, rupturing them may have the effect of checking the hemorrhage, by allowing the uterus to contract on the vessels from which it was flowing, just in the same manner as takes place when they are ruptured in accidental hemorrhage. In regard to Barnes's operation, I cannot imagine any case in which it would be justifiable. (Dr. C. Bell, p. 268.)

POST-PARTUM HEMORRHAGE.—*Prophylactic Treatment.*—If the labour be sufficiently advanced, and the pains, from any cause not within the scope of a digital examination, begin to hesitate, and finally disappear, I invariably administer a full dose of Richardson's liq. secalis ammon., which can be repeated in about half an hour, provided one dose has failed to stimulate the uterus into action. This is a beautiful preparation of ergot, and is not disliked by patients; it is void of the *nauseating* effects of other preparations, and has the advantage of being more active and reliable in its operation. It is conveniently given in a cup of tea, and, from the presence of free ammonia, is slightly stimulating beyond its specific effects. Like other preparations of ergot, it will produce "cramp" if given to excess, and before the os is sufficiently dilated—*i.e.*, if given too early in labour. I have given it as a mixture every two or four hours (depending on

the urgency of the case) in various miscarriages with undue "loss," and it has seldom failed to expel placental débris, clots, &c., securing contraction, and cutting short the hemorrhage. As I have stated, it does not sicken, and at present I do not know a better therapeutic agent in simple uterine inertia. Therefore, I have no hesitation in pronouncing it a valuable addition to our means of averting and limiting post-partum and other uterine hemorrhages. But we cannot rely on ergot alone. In order to keep up continuous uterine contraction, the left, or disengaged hand, should be planted firmly on the fundus uteri, which should be followed in its descent as the child clears the lower outlet. As both hands are likely to be engaged at this stage of the labour, the hand of a nurse should be placed in the same position, and if not frequently reminded she will be pretty certain to relax her hold. No time should be lost in separating the child, and replacing the hand on the partially emptied, and consequently less perfectly supported, uterus. Squeezing should not be immediately practised with a view to expelling the placenta, which, as a rule, will soon separate and be found in the vagina or the bed. This is indicated by a further recession of the fundus uteri, which is now low in the pelvis. Attention should be given to the pulse, and if about 100 per minute, another dose of ergot should be given, and the result carefully watched. A steady pulse of 80 or 90 after the lapse of an hour need give no alarm. (Dr. A. McCook Weir, p. 261.)

Post-partum Hemorrhage from Inertia.—Not unfrequently the fault lies in the uterus itself, owing to inertia. In order to relieve this condition various means have been advocated, all of which have been successful, and all of which have failed, the chief aim and object of all being closure of the mouths of the bleeding vessels, either chemically or mechanically. Could we but get the uterus firmly, equably, and permanently contracted, hemorrhage could not occur, and the converse of this is equally true. Nature has adopted a beautiful means for the prevention of hemorrhage from the veins and sinuses of the uterus—which is a hemorrhage by retrogression—by the position and mechanism of the uterine veins, although devoid of valves. This natural mechanism was beautifully shown in a case of dissection of a gravid uterus about the sixth month, which came under the observation of Mr. Owen, who states that he "observed that where the veins of different planes communicated with each other in the substance of the walls of the uterus, the central portion of the parietes of the superficial vein invariably projected, in a semi-lunar

form, into the deeper seated one, and where (as was frequently the case, and especially at the point of termination on the inner surface) two, or even three, of these wide venous channels communicated with a deeper sinus at the same point, the semi-lunar edges decussated each other, so as to allow only a very small part of the deep-seated vein to be seen. It need scarcely be observed how admirably this structure is adapted to insure the arrest of the current of blood through these passages upon the contraction of the muscular fibres with which they are everywhere surrounded." (Dr. M. M. Bradley, p. 254.)

Hot Water Injections in Post-partum Hemorrhage.—For arresting uterine hemorrhage hot water must soon supersede all drugs and medicated injections, being superior to them all in many important particulars. 1. It is easily attainable at all times. 2. It is absolutely safe, if care be taken to exclude air from the syringe. 3. It stops hemorrhage not by artificial plugging, but by causing a natural contraction of the uterus. 4. It is cleanly, and a disinfectant, such as carbolic acid, can be easily added. 5. By imparting heat, it rallies the exhausted patient, and gives power to the muscles for contracting, instead of, as is the case with ice, abstracting what little heat remains, and so benumbing and paralysing them. Can more be required of an agent? It is my practice to have a syringe and hot water always on hand. In case of hemorrhage the water is used as hot as can be borne by the hand. If, however, all goes well, a vaginal bath is given at 100° F., and continued for several days night and morning. A few drops of the strong impure carbolic is added to the water as a disinfectant. (Dr. Wm. S. Whitwell, p. 259.)

PREGNANT SICKNESS.—*Oxalate of Cerium.*—The one or two grain doses of the oxalate usually given are perfectly useless, but I have not met with a case in which the nausea has not been very considerably relieved, and in most cases completely checked by ten-grain doses of the oxalate of cerium. The oxalate of cerium I have also found most efficacious in retaining the nausea resulting from uterine irritation. I generally combine it with bromide of potash in these cases. (Dr. F. E. Image, p. 271.)

PUERPERAL FEVER.—Puerperal fever is indeed nothing else than the infecting of fresh wounds, such as are found in every newly-delivered woman, with destructive septic materials. Now, materials of this sort gain admission in two ways: first, and this happens more especially in very difficult and long labours, under the influence of the particles that

cause putrefaction which are ever present in the air and ever ready to press in, decomposition occurs in the mother's own secretions and excretions, and thus takes its rise in the maternal organism itself; or, secondly, these materials are introduced into the female genital canal *from outside*. This latter is brought about almost exclusively by the finger or instruments of those who examine the lying-in woman—that is to say, of the midwife or the physician. If the instruments or finger of these persons have not been cleansed with the greatest care, and disinfected most conscientiously after they have been in contact with any infective matters, the result is that these matters are brought into contact with the fresh wounds of the woman during labour or subsequently, and thereby infect her with a fatal disease. A specially frequent channel of infection is from a diseased lying-in woman, to another, because midwives quite commonly from a diseased lying-in woman, whom they are nursing, attend upon a new confinement, without sufficiently purifying themselves; and it cannot be doubted that, in the majority of cases, the midwives are the carriers of this infection, because, to their exclusive care the great majority of labours is intrusted, and they naturally come into more intimate relations with the lying-in women than the physicians, who, in very many cases, are only called in after the onset of threatening symptoms. (Report of Berlin Obstetrical Society, p. 281.)

RECTAL EXAMINATION OF THE UTERUS.—If the thumb of one hand in the vagina fixes the vaginal portion of the cervix uteri, the index-finger of the same hand in the rectum can not only feel the posterior surface of the uterus distinctly, but can follow the sacro-uterine ligaments; while, if the other hand presses the abdominal wall backwards towards the sacrum, a very accurate idea can be obtained of the relations of all the pelvic organs. The uterus can be moved in various directions, and anything between it and the bladder or rectum is distinctly felt, supposing of course no extraordinary amount of fat in the abdominal wall, nor any peculiar rigidity in the vagina, interfere. Flexions of the uterus are thus very accurately recognised, and often replaced easily. (Prof. Hegar, p. 284.)

TEDIOUS LABOURS.—*On the Use of the Forceps.*—The instrument I use and recommend is so intimately connected with what I consider distinctive in my practice, that, without such an instrument it is impossible, if it should be considered desirable, to apply it as I have done, for I find I almost invariably place the blades antero-posteriorly, or nearly so. The advantages of grasping the head in this way I think, and

have attempted to show, are enormous, and I feel confident, from an experience of half a lifetime, that the practitioner who once gets and uses the forceps as I have directed will never feel that he needs another. Get from Mr. Young, of Edinburgh, who made mine, a forceps such as I have described, and I think you will find it the best investment in instruments you have ever made. Get, I would say, the three blades, for though I have generally managed to dispense with the short blade, it gives a good hold where the os sacrum is very prominent, and it can be laid aside when the labour advances. The stronger instrument I have latterly used possesses decided advantages over that originally designed by Dr. Ziegler, from its greater length and strength, and from other circumstances which I have mentioned. The hold which I get is generally perfect. The solid blade also seems to me to be an improvement, for there is no catching in introducing it or in pulling with it, and there is not even a scratch left where it has been fixed. Two of my recent cases had the face to the pubes, and the solid blade was at first in both applied over the face, without the slightest injury to the skin. Though it may be necessary occasionally to introduce the forceps *within* the uterus, try to avoid this as much as possible by assiduously pushing up the uterus with the fingers or hand both anteriorly and posteriorly, for the application of the instrument is thus made easier, and the risk to the mother is lessened. (Dr. G. Hamilton, p. 241.)

MISCELLANEA.

ALCOHOL.—*Action of.*—The peculiar way in which alcohol transudes through membrane, its diffusive power as it is called, enables it to reach all parts of the body in an extremely short space of time. There is dilation with reduced power of contraction. This vaso-motor paralysis, so to speak, is more continuous as well as more complete in some organs than in others, and some forms of alcoholic drinks produce it much more certainly than others. This is the case if the alcohol be impregnated with some of its allies, especially amylic alcohol and fusel oil; these latter are always more or less present in potato spirit, with which wines are commonly fortified. The influence of these alcohols is very decided upon the nervous centres; they immediately produce some of those exaggerated forms of drunkenness in which violent excitement and maniacal delirium are manifest. They make the fluid pleasanter to the taste; and some of their forms are frequently added for the purpose of producing a so-called "bouquet." Their effects

are far more serious and immediate than those which follow from the simple use of ethylic alcohol. In its first origin the effect is to interfere with the proper renovation and nutrition of the part, and to commence a disease of the particular organ affected. Dyspepsia is the first outcome of the use of stimulants, and as a sequence to a common result, viz., inflammation of Glisson's capsule, we may have so-called cirrhosis or fatty degeneration of the liver, and similar changes may take place in the kidney; the heart and large blood vessels may suffer directly or indirectly; the muscles may be destroyed, or the nerve-tissue of the brain or spinal cord may be the parts which show the effect of the reagent first. (Dr. A. Carpenter, p. 323.)

BEEF-TEA.—In making beef-tea it is well not to add the water to the meat until the latter has discharged all the gravy from its fibre under the influence of moderate heat. Cut or tear an ordinary beef-steak to pieces on a dish, using a knife and fork, and reducing it to somewhat small fragments; then throw the meat into a jar, and close the top with an earthenware cover, or paper stretched over it, and tied. In any case keep in the steam. Place the jar in a slow oven, or set it on the top of a boiler, or, still better, stand in a saucepan of hot water, *not boiling*. Be careful that no water reaches the meat. After standing for about three-quarters of an hour, exposed to a moderate temperature, hot enough to melt the soft parts of the meat, but not to coagulate it, remove the cover, and pour on water below the boiling point in the proportion of one pint to a pound of the meat; add a little salt, and let the contents of the jar now simmer an hour, and strain through a coarse cloth, squeezing the residue, which will be found to consist of indigestible fibre, while the "beef-tea" is strong and good. (A Practical Physician.)

With reference to the preparation of beef-tea, I would suggest the following recipe to the consideration of your readers as one which furnishes the most nutritious, and perhaps the least expensive, aliment of the kind for invalids:—Take one pound of *neck* of beef, chopped very finely so as to resemble "sausage-meat," and add to it one pint of cold water; set aside, and allow it to soak for twelve hours; then gently and steadily heat, or, what the cook calls, simmer, for two hours; strain through a coarse sieve, a cullender, and it is ready for use. For those, however, who have a great distaste for fat, or with whom it appears to disagree, the beef-tea must be allowed to cool, and the fat removed from the surface. It may be flavoured with mace, nutmeg, pepper, &c., according to taste. (Dr. T. F. Pearce, Lancet, June 15, p. 887.)

BUTYL CHLORAL.—*Its Action.*—The former name croton-chloral is quite incorrect and very misleading, as it has no relation to croton-oil. The first action of the drug is to produce anæsthesia, which begins in the head and gradually passes to the rest of the body, reflex irritability remaining intact in the limbs for some time after it has been abolished in the head. Then follows narcosis; but it is important to observe that anæsthesia in the head and face may be complete before any trace of narcosis manifests itself. From a consideration of this fact Professor Liebreich suggests the use of butyl chloral in operations on the face, in cases where other anæsthetics are contra-indicated. The dose for this purpose is from one to two grammes (15 to 30 grains). The chief interest of the lecture lay, however, in the remarks made as to the action of butyl chloral on the heart, and the manner of death in fatal cases. It was pointed out that whereas in the case of chloral hydrate the heart was paralysed and its action ceased before the movements of respiration came to an end, the reverse held good in the case of butyl chloral. A fatal dose of that substance attacks the respiratory centre in the medulla oblongata and paralyses it, while the heart is unaffected and continues to pulsate for some time after death. (Prof. Liebreich, p. 306.)

CARBOLIC ACID.—*Medical Uses of.*—The diseases in which carbolic acid is especially useful are. 1. All that class of local festering, pustulating diseases of the skin which are at once so common and so difficult to cure. They include all kinds of pustules, boils, and carbuncle; sycosis, pustular acne, and festering ringworm. 2. Such strumous sores (especially of the neck) as come under the care of the physician. 3. Excoriations of the os and canal of the cervix uteri. 4. Phthisis in its second and third stages, and cases of chronic bronchitis accompanied with more or less purulent expectoration. In order to be effective the carbolic acid must be brought into contact with the part to be acted on, and in many cases where it has been found ineffective the failure has been due to a neglect to ensure this contact. In the pustulating and suppurating diseases of the skin it is never sufficient to apply the solution of the acid, of whatever strength, upon or to the outside of the skin. It must always be introduced into the interior of the sore or pustule itself, and so as to come sufficiently in contact with every part of the diseased surface. All cases of boils and carbuncles in their earlier stages can be absolutely aborted and cured, whilst even in later stages their further increase can be almost surely prevented. For this purpose a very strong glycerine solution should be employed, and it is best conveyed into the interior of the

pustule, boil, or suppurating spot, by a new quill pen dipped into the solution, and introduced by a rotatory motion through its apex, where a sufficient aperture will generally be found. In carbuncles, which are necessarily larger, and have often several openings, several such introductions may be necessary, or, at a later period, threads of lint soaked in the fluid, may be passed with a probe well into all the sieve-like openings. Occasionally, as when the mass is large and solid, a watery solution of the acid may be injected with a hypodermic syringe into various parts of the hardened growth. The same plan of treatment is often quite effective in cases of sycosis, pustular acne, and festering ringworm. (Dr. P. Eade, Lancet, June 29, p. 927.)

DIRECT METHOD FOR RESTORING THE APPARENTLY DROWNED.—

1st. INSTANTLY turn the patient downwards, with a large firm roll of clothing under the stomach and chest.

Press with your weight two or three times, for four or five seconds each time, upon the patient's back, so that the water is pressed out of the lungs and stomach, and drains freely downwards out of the mouth. Then

2nd. QUICKLY turn the patient face upwards, the roll of clothing put under his back just below the shoulder-blades, the head hanging back as low as possible.

Place the patient's hands together above his head.

Kneel with patient's hips between your knees.

Fix your elbows against your hips.

Now, grasping the lower part of the patient's chest, squeeze the two sides together, pressing gradually forward with all your weight, for about three seconds, until your mouth is nearly over the mouth of the patient; then, with a push, *suddenly* jerk yourself back—

Rest about three seconds; then, begin again.

Repeat these bellows-blowing movements, so that air may be drawn into the lungs, about eight or ten times a minute.

Remember, the above directions must be used *on the spot*, the instant the patient is taken from the water. A moment's delay—and success may be hopeless. As soon as the water is pressed from the lungs, all clothing should be ripped away from the chest and throat. In making the pressure, either for the removal of water or for breathing, increase it *gradually* and thoroughly, and *suddenly* let go with a jerk. With women and children use less force.

Do not stop these movements under an hour, unless patient breathes. Be careful not to interrupt the first short natural breaths. If they be long apart, carefully continue between them the bellows-blowing movements as before.

After breathing is regular, keep patient warm with blankets, rubbing with warm hands, &c.

Prevent crowding around patient; plenty of fresh air is all-important.

Spirits and water only, in occasional small doses, may now be given; if hot the better. After this, encourage quiet and sleep. (Dr. Benj. Howard, p. 315.)

IMPURE AIR IN MINES.—*Apparatus for rendering Impure Air Respirable.*—Dr. Ball, of the Blaenavon Iron Works, describes an instrument for rendering impure air in mines respirable. (See woodcuts pp. 309, 310.) He says, "by the use of this apparatus, I expect that a collier would be enabled to go into a mine charged with carbonic acid. In the case of an explosion, in which all the oxygen had been converted into carbonic acid and steam, the explorer would be enabled to supply himself with such an amount of oxygen as would neutralise the effects of the carbonic acid. The man could easily carry a supply of oxygen which would last for two hours, even when he was using the maximum quantity required; and, by connecting a second tube and a face-piece to the oxygen-reservoir, he could revive any sufferer whom he might find overcome by carbonic acid." (Dr. C. B. Ball, p. 309.)

MILK AS A VEHICLE FOR QUININE.—It is not generally known that milk is an elegant and convenient solvent for quinine, and that it disguises to a great extent its bitterness. If one grain of sulphate of quinine be dissolved in an ounce of milk, we shall find that the bitterness of the draught is hardly perceptible; with two grains there is rather more bitterness, but it is not at all marked. A dose of five grains may be taken in two ounces of milk without an unpleasantly bitter taste; and if the same quantity be put into a tumblerful of milk, the bitterness is all but lost. This method of administering quinine must in some cases be preferable to the ordinary way of dissolving it in acid or spirit, especially where the bitter taste is objected to (as in the case of children), or where the required dose is large; and it will, doubtless, be found to possess other advantages. (Dr. R. L. Batterbury, Berkhamsted, Brit. Med. Jour., June 29, p. 933.)

NUTRITION.—*Phosphates.*—The *inorganic* materials in the soft tissues, and their relative proportions, exert a very important influence in nutrition; and their absence or presence in insufficient quantities, either from diminished supply or imperfect assimilation, is the origin of a class of diseases (organic) which have hitherto received no adequate explana-

tion as to their cause. The only form of phosphorus met with in food and in the animal tissues is phosphoric acid. The seat of phosphates in the tissues is in the capillaries. The capillaries, including arterioles, capillaries proper, and venous radicles, have to do with the building up of tissues; and it is essential to keep in view, that the inorganic constituents of food, the phosphates among the rest, are as essential for perfect nutrition as the flesh-formers and heat-producers. (Dr. M. F. Anderson, p. 334.)

STRYCHNIA AS A TONIC.—It would appear that strychnia or nux vomica is one of the most valuable tonics which we possess. When combined with nitro-hydrochloric acid it is perhaps one of the most efficient remedies that we can give for the debility which is so often noticed in warm weather, and when the ordinary tonics, such as gentian, columba, cascarilla, or quinine do not produce the desired results, the addition of a little nux vomica or strychnia to them may give us the wished-for effect. (Dr. T. L. Brunton, p. 340.)

PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

ART. 1.—ON THE INTIMATE NATURE OF INFECTION AND CONTAGION, AND THE PREVENTION OF ZYMOTIC DISEASES.

By Dr. THOMAS WRIGLEY GRIMSHAW, M.A., Lecturer on Medicine in Steevens' Hospital, Dublin.

The terms zymosis and zymotic have been used in medicine from the most ancient times down to the present day. It is scarcely necessary to mention that zymosis means fermentation, and that it is derived from the Greek ζυμε, leaven—hence the term zymotic disease, which has been applied to certain diseases upon the supposition that the processes by which these diseases are developed bear close analogy to the process of fermentation, as observed in the fermentation of saccharine fluids or vinous fermentation. I wish to lay special stress upon the fact that it was the character of the vinous fermentation alone that suggested the application of the term zymotic to diseases, as, when it was first introduced into medicine, the vinous was the only fermentation studied, even if others were known; the term zymotic disease is now used as synonymous with infective disease. The use of the term zymotic is one which, I believe, has done more to retard the progress of the study of the diseases to which it is applied than most of the other numerous pathological misnomers with which we are troubled, and the use of the term has, I think, tended to impress upon the minds of even scientific observers the idea that observations upon fermentation of various kinds will determine the nature of the processes which take place in some of the infective diseases. I shall now discuss, as briefly as possible, the amount of light which the pathology of the infective diseases may derive from the study of fermentation. Probably the best definition of the term "fermentation" is that given by Professor Miller—namely, "Various transformations which organic compounds experience under the influence of a small quantity of organised matter, which is itself in a state of active alteration." "This active substance, which is termed a ferment, neither imparts anything to nor receives anything from the substance which is undergoing fermentation."

In the vinous fermentation we find that the introduction of the yeast plant into a saccharine fluid determines the fermentation of such a fluid, and the conversion of the sugar contained therein into alcohol and carbonic acid. The yeast plant will not increase nor multiply in such a fluid, if it contain sugar only. The sugar will ferment but the plant will die. For the plant to continue its existence, a proper soil for it to grow upon must be provided, and this soil must contain nitrogenous elements to nourish the tissues of the yeast plant; but such growth and such pabulum is unnecessary for the production of fermentation alone. If the yeast plant be washed with distilled water, its power of fermentation is diminished—indeed, it may be temporarily suspended, although the life of the plant is not destroyed. It thus appears that the fermentation is caused by a product of the growth of the plant—*not* by the plant itself. That an active principle, capable of itself producing powerful effects on other bodies, can be developed during the growth of such tiny organisms as the yeast plant, is just what might be expected when viewed by the light of the analogous results which take place during the growth of higher vegetable organisms. Thus the various poisons existing in vegetables are the products of the growth of the plants from which they are obtained. These agents, when separated from the plants, are not in any sense living, but are capable of producing as direful results upon human beings, the lower animals, or in some cases even upon plants, as any which have been attributed to disease germs. Some of these active principles may even be produced artificially.

I do not propose to discuss the various forms of fermentation, which seem now to be almost innumerable, but wish to call your attention to a process, strictly homologous to, if not identical with, fermentation, which has a most important bearing upon the question of the infective processes of disease.

There are certain little organisms whose name, "bacteria," is now in the mouth of everyone. They are as generally talked of as if those who discuss them were intimately acquainted with their appearance and nature. The name, in consequence of sensational lectures, has come to be commonly employed in the same sense as disease germ; so "bacteria and germs" are now looked upon as almost equivalent to "plague and pestilence." Now bacteria in themselves appear for the most part to be harmless little creatures; they or their germs swarm in millions in the air we breathe, the fluids we drink, and the food we eat. They can be collected from our mouths at any time, and when washed clean by distilled water have been injected into the blood without producing any evil result. It is quite clear that if bacteria were possessed of the terrible powers attributed to

them they would soon have the world to themselves, having eaten up or decomposed all other organised beings. In spite, however, of the apparent harmlessness of these bacteria, it seems now to be almost certain that their existence is necessary, though their presence not essential, to the production of some of the most terrible forms of zymotic or infective diseases with which we have to deal. One disease (anthrax) is said to depend upon them (Pasteur). It is at this point that the study of fermentation throws important light upon the nature of some forms of zymotic disease; it is because bacteria take part in a certain fermentation that they are important from one point of view. It is now agreed by all investigators that if septic material—that is, decomposing animal matters—be introduced into the blood of an animal, it produces poisonous effects. The result may be immediately fatal, or the effect may be slight and the animal recover, but of the effects of this poisoning I shall have more to say hereafter. A poisonous septic fluid may be prepared by infusing animal tissue (*e.g.*, muscle) in water, and when this becomes putrid, boiling it with alcohol, removing the alcohol by filtration and evaporation, and extracting it with water. This fluid contains septic poison, capable of destroying life, yet whatever the poison be, it has not been destroyed by boiling alcohol. It has been proved by a well-devised and beautiful series of experiments that the poisonous fluid contains neither bacteria nor germs of any kind; the poisonous matter can, however, be filtered out by a porcelain filter. Now, having ascertained that the fluid containing the poison is free from germs, it is necessary to find out the source of the poisonous material. It arose from the putrid infusion of animal tissue, but how did it get there? It may be now considered as proved, by the observations of Dr. Burdon Sanderson and others, that this virulent poison is the product of a fermentation in the infusion of muscle, the fermentation being promoted by the presence of bacteria, just as the vinous fermentation is promoted by the yeast plant; the ultimate result in the former case being the septic poison—in the latter, alcohol, or, as we may fairly call it, the alcoholic poison. I must here mention that the bacteria, which first appear in organic infusions or cultivating fluids, do not possess the power of producing a septic poison, but the succeeding generations produced by these bacteria do possess this power. The first generation of bodies do not present the same appearance as those which follow, bacteria having the appearance of little rods, produce as offspring, not rod-like bodies, but spheroidal bodies, called micrococci, which have a tendency to collect into masses (glœa) in which the bodies appear to cohere by means of a gelatinous substance. So much then for the relation of fermentative processes to the

production of disease, and it amounts simply to this—that certain fermentations generate poisons, which in their turn may produce diseased conditions.

Spontaneous Generation.—The next question to be considered is the relation of what is termed "spontaneous generation" to the production or promotion of disease. Spontaneous generation has been and usually is taken to mean "the production of living things from materials not previously endowed with life; the term is objected to by Dr. Bastian under the belief that any operation performed either by the action of physical or vital *laws*, cannot be considered spontaneous. He has proposed to employ the term "archebiosis." While I quite agree with Dr. Bastian, yet I think it better to employ the more familiar term "spontaneous generation."

I do not come before you, however, as an advocate of spontaneous generation—on the contrary, taking an impartial view of all the evidence yet produced for or against the theory, I am distinctly of opinion that at present the supporters of this theory have got the worst of the discussion, and must produce further evidence and new facts to prove their case.

Every one is familiar with the fact that a beam of sunlight, falling into a comparatively darkened room, has its course marked by a cloud of floating motes. The individual particles which constitute this cloud have been investigated by Professor Tyndall, the Rev. W. H. Dallinger, and others. The rough method of examining the track of a sunbeam in a room has, of course, to be abandoned, and a method devised by which particular portions of air can be tested by bright light. Professor Tyndall has provided such an arrangement in the form of a glass chamber, through which he can pass, at will, a beam of electric light. If such a beam be permitted to fall through the air of an ordinary room, the beam is marked by a luminous track, caused by the illumination of the numerous floating particles by the beam. If these particles be in any way removed, the track of the beam ceases to be luminous. The particles can be destroyed, even in the atmosphere of an ordinary room, by the simple expedient of burning them with the flame of a spirit lamp, or a red-hot poker, when the portions of the beam exposed to the heat cease to be luminous. If ordinary air, drawn from any room, be enclosed in a glass chamber, the electric beam presents luminosity. If however, the air be filtered through cotton wool before admission to the chamber, the electric beam cannot be tracked through it. If the chamber, filled with air containing the particles I have referred to, be allowed to remain at perfect rest for some hours, it will be found that the air, when examined by the electric beam, has become as completely purified from dust as if it had been filtered before

admission to the chamber. The dust, however, is not *gone*, but has only subsided to the bottom of the chamber. Dallinger has found that the rapidity of the settlement of the particles is in proportion to their weight. Fluids may be tested in the same manner for floating particles, and they will subside to the bottom of the fluid. What bearing have such experiments upon the question of spontaneous generation? It is affirmed, and, I think I may say, proved, that the cloud in the air and in the fluid, demonstrated by the electric beam, contained not only inorganic particles and organic dead materials (such as pieces of clothing, hair, cuticle, dead insects, &c.,) but also the germs, if not the developed forms, of low, but living organisms; and the proof consists in this, that when any air or fluid, capable of rendering the electric beam luminous, is brought into contact with any organic fluid or an artificially prepared cultivating fluid (which we may call a nutrient fluid), there will be produced in such fluid well-recognised organic forms. On the contrary, if the air or fluid be not capable of rendering the beam luminous (the particles having been removed by filtration), such air or fluid, when brought into contact with a nutrient fluid, will not develop any living organisms, as it is what is technically termed "sterile." This is the assertion of those who disbelieve in spontaneous generation, and all their assertions have been substantiated by experiments (Pasteur, Tyndall, Dallinger, &c.) In opposition to the above view, the believers in spontaneous generation affirm that certain materials, each and all of which have been so treated as to render the presence of germs therein perfectly impossible, will (when mingled) produce living organisms. The chief investigator and supporter of this doctrine is Dr. Bastian, whose well-known abilities as a physiologist and pathologist entitle his opinions and the results of his experiments to the highest consideration. Dr. Bastian's experiments consist in the taking of certain fluids, exposing them to a boiling temperature, hermetically sealing them in glass tubes, and then showing that, without any contact with the contaminating influence of the atmosphere, living beings can be developed therein, if the materials necessary for their nourishment exist therein. Time does not permit me to detail the steps of Dr. Bastian's experiments. The main feature in all the arguments for Dr. Bastian's observations is the assumption that a temperature of 212° will destroy life, if such existed in the fluid experimented upon. Dr. Bastian's conclusion is, "that specks of living matter may be born in suitable fluids, just as specks of crystalline matter may arise in other fluids." There does not appear to me to be any reason why this may not be correct, but I do not consider that Dr. Bastian has sufficiently proved it. The most recent experiments of Tyndall and Pasteur,

Dr. Bastian's great opponents (and also of Dallinger), have proved that Dr. Bastian's assumption of the destruction of life at the temperature employed by him for that purpose is incorrect. In fact Tyndall and Pasteur have pretty well disposed of all the arguments in favour of spontaneous generation founded on the belief of certain temperatures destroying life. It is admitted that bacteria and many other low forms of organic life will be destroyed by high temperatures, but these are the complete organisms—not their *germs*. Pasteur proves that the germs are more resisting than the fully-grown organisms. This is not a surprising result, though one requiring great perseverance and trouble to prove. We know that the ova and even the partially developed embryos of the higher animals live under circumstances totally incompatible with the existence of their parents, or the beings into which these "germs" or embryos are ultimately to grow. Supposing all I have said to be correct and the conclusion true, what is the present state of knowledge with regard to spontaneous generation? Simply this—that, up to the present, spontaneous generation has not been proved. It has been proved that life may exist under the most adverse circumstances, and under conditions which were supposed to preclude the existence of living organisms. At the same time the possibility of spontaneous generation has not been disproved, although the probabilities are against its existence. Now, what bearing has this upon the origin of zymotic diseases? This, that if diseases necessarily depend upon germs, then, if we destroy the germs or their parents before they enter our bodies, we prevent disease—if we kill them after they enter, we stop or retard disease. If they are spontaneously generated they may possibly arise within the body, and precaution outside the body is useless. In any case, if it is admitted that germs are essential, their destruction will either prevent or cure disease.

It is now my duty to consider the question of "septic infection," or "septicæmia," as it is usually termed. The word "septicæmia" is a bad epithet for the condition which arises from septic infection, because it implies putridity of the blood, in the sense of putridity outside the body; and thus a word wrongly applied has led to more argument about the word than about the condition which it is supposed to describe. The word does not convey to pathologists the idea of putridity of the blood, but of blood poisoned by a certain substance derived from a putrid material. In my remarks on fermentation I pointed out that the fermentation of certain organic fluids gave rise, as a result, to a poison called septic poison; we have now to consider the means by which this poison may be introduced into the blood, and the results which may follow its introduction.

It has been shown that where bacteria exist this poison is liable to be produced, therefore if a suitable soil for the maintenance of the life of the bacteria and their progeny is maintained in any portion of the body where entrance to the blood stream can be obtained, the system is in danger of the septic poison. Now such soils for the growth of bacteria exist in wounds, some forms of ulcers, anthraces, and the uterus of a recently delivered woman; the presence of bacteria in such stations is a mere matter of observation, and does not depend on opinion.

The points which I particularly wish to emphasise are:—1st. That the septic poison may be produced *outside*, and not even in contact with the body which it affects. 2nd. That it may be produced within the same body, provided a suitable soil for the growth of bacteria and their progeny exist therein. 3rd. That the septic poison is incapable of self-multiplication. Bacteria or their germs may enter the system by the lacteals, by the lungs, or through wounds. Healthy blood never contains bacteria or their germs, but it is proved that healthy organs do contain bacteria, therefore the bacteria and their germs are capable of being destroyed by the vital processes; indeed if it were not so life would be impossible. If, however, septic germs be introduced into the circulation of an animal suffering from inflammation produced by an injury, such as the operation of *bistournage*, the germs will lodge in the inflamed part and produce diffuse inflammation, which quickly extends. In a similar manner germs from the intestine may make their way to a suitable soil in an inflamed peritoneum, and produce similar and fatal results. These poisoned soils become thus centres of true septic poison. We can here see how an inflamed or irritated peritoneum may become the means of diffusing fatal infection.

[The following are the writer's conclusions as to the present state of our knowledge.]

A.—With regard to fermentation—1. That certain kinds of fermentation, in which bacteria take part, are capable of developing a poison, called the septic poison, which produces specific effects when introduced into the circulatory system. 2. That the septic poison may be produced independent of the living body, or may be elaborated in wounds by the action of bacteria.

B.—With regard to spontaneous generation—1. That the spontaneous generation of living organisms has not yet been proved. 2. That spontaneous generation is not impossible. 3. That in the absence of proof or disproof of spontaneous generation, we must act as if such a process were possible.

C.—With regard to septic infection—1. That the septic poison

produced by bacteria and their progeny, when introduced into the system, produces disintegration of the blood corpuscles, consequent coagulation of the blood, and thus stasis and capillary congestion. 2. That the effects of the septic poison are in direct proportion to the dose.

D.—With regard to the contagion of specific diseases—1. That the contagia of specific diseases are particulate and living. 2. That these contagia closely resemble vegetable organisms in their growth and development. 3. That up to the present the specific contagium of any disease has not been demonstrated. 4. That in several contagious diseases certain organisms have been found which are constantly related to those diseases, and must therefore be considered to be either the contagia of such diseases or the only carriers of such contagia.

Practical Application.—I shall now proceed to make some practical application of the knowledge which I have shown we possess upon this subject.

With reference to *septic infection*, assuming that it is true that the septic poison is the product of the growth of bacteria, it is quite evident that any agent which will kill the bacteria or their germs before they have developed the poison will, as a matter of necessity, prevent the occurrence of septic infection. Now, I have shown that septic poison may be produced outside and at a distance from the body which it ultimately affects; and that, further, all the conditions favourable to the growth of bacteria, and therefore to the production of septic poison, exist in wounds and sores, and that the intestinal tract itself contains bacteria capable of producing septic poison in the neighbouring parts if the proper soil is provided. We at present know of no antidote to the septic poison, and our dealings with it, when once it enters the system, consist in maintaining the life of our patient until the effects of the poison have worn off, or combating the various secondary conditions which septic infection gives rise to. We may, however, prevent the entrance of the poison, or even prevent its formation, under certain circumstances. The greatest difficulty with which the surgeon has to contend is the septic infection of wounds. The results of the most carefully devised and beautifully executed operations have been frustrated by septic infection. I think I have shown conclusively that ordinary air contains, among many floating objects, the germs of the lowest forms of life, and among these bacteria and their germs. This being admitted, it follows that a wound exposed to the influence of ordinary air is at the same time exposed to the danger of becoming a field for the growth of bacteria; and if these bacteria settle upon the surfaces of the wound and there produce their venom, the

patient will as certainly suffer as if the septic poison itself, produced from a putrid infusion, had been introduced into the wound. The avoidance of the introduction of matter known to be putrid, and therefore possibly containing septic poison, is a mere matter of scrupulous cleanliness, and the carrying out of such cleanliness is well known to be a source of great success in surgery. All putrid matters should, therefore, be removed far from the healthy, and, still more, from the sick; and in public institutions and hospitals should be destroyed by fire or powerful corrosive chemical agents. We cannot, however, apply corrosives or fire to our patients and their wounds, and, fortunately, here we have the means of preventing the formation of septic poison by the use of antiseptics. These antiseptics act, not by destroying the poison itself, but by destroying the agents which produce the poison. The poison has been shown (Dr. Anders) to be capable of withstanding the most powerful antiseptics.

When speaking of fermentation, I cited the case of putrefaction as instance of fermentation. It is right here to state that some authorities have endeavoured to distinguish between fermentation and putrefaction, defining the former as changes connected with the growth of "torulæ" (yeast plants), and the latter as changes in connection with "bacteria." Now this I consider quite an artificial and misleading distinction, and one which, if followed out, would lead to the giving of special names to a variety of processes almost identical with torulous fermentation. This attempt to distinguish between fermentation and putrefaction seems to have followed on the discovery that certain agents which control tortulous fermentation do not control bacterial fermentation. It is with bacterial or septic fermentation that we have to deal when considering the question of antiseptics, which are agents fatal to the growth of "bacteria." The test of the power of an antiseptic is the amount of control it has in preventing the occurrence of septic fermentation in a cultivating fluid. Experiments carried out by Dr. Baxter, published in 1875, have shown the relative antiseptic powers of different agents. Dr. Baxter's method of taking antiseptic power is as follows:—"A known portion of the agent under investigation was added to a liquid teeming with septic microzymes. A test solution, previously sterilised by heat, was then inoculated with a minute drop of the disinfected liquid. If it continued barren, the successful destruction of the septic germs is proved. In the event of their incomplete destruction, the test liquid was found crowded with their progeny." The result of Dr. Baxter's experiments was to prove that—

	Strength per cent. or more.	
Potassic permanganate	0·007	destroyed the reproductive power of microzymes.
Chlorine - -	0·0008	
Sulphur dioxide -	0·123	
Carbolic acid -	1·	

The antiseptic action of the permanganate and chlorine is seriously impaired by the presence of albuminous compounds, whereas the action of sulphur dioxide and carbolic acid are scarcely, if at all, so affected. Hence in dealing with animal fluids, the two latter are the valuable agents, and the two former nearly useless.

Professor Lister, by the careful and assiduous applications of these scientific facts to practical surgery, has devised a method of treating wounds now known as "antiseptic surgery," or, as it may be fairly called, "Lister's method," for to him belongs the credit (in spite of arguments about priority of suggestion) of having really applied the science of antiseptic to practical surgery. I cannot enter upon the details of Lister's treatment of wounds, but it consists in taking measures to destroy as completely as possible all bacteria and their germs, before either they or their septic venom have established themselves in the wounds produced by operations and accidents. This is accomplished by surrounding the patient with an antiseptic atmosphere, bathing the wounds with antiseptic fluids, and forming a screen of antiseptic material to prevent the access of microzymes to the wounded part. I am aware that antiseptic surgery is not always successful, and that it is stated that microzymes have been found behind Lister's antiseptic screens. I am also aware that very successful results have been obtained by other means than those adopted by Lister. Antiseptic surgery has been called a system merely of cleanliness; be it so, the dirt to be dealt with is dangerous, and moreover *living*. Lister has proved that he can with comparative ease destroy this dirt, and can show why it is dangerous, and that it is destroyed by depriving it of life. This is undoubtedly the most efficacious way of disposing of these pests of surgery, and thus destroying septic infection and its possible results—pyæmia and hospital gangrene. The experience of Continental surgeons has fully confirmed Lister's views and practice. So far the practical application of our knowledge with regard to septic fermentation and the production of the septic poison is found in antiseptic surgery. May not the physician also apply this knowledge to the prevention of septic infection from the ulcers of enteric fever, the sloughing sores of the throat in scarlatina or diphtheria, or the condition of skin in the advanced stage of

small-pox? I believe he can, though perhaps to a more limited degree than the surgeon, yet the practice of the surgeon in this as in many other cases may serve as a useful guide to the physician.

We have now to consider the application of our knowledge to the prevention and cure of specific contagious zymotics. I cannot consider each disease of this class, but I shall endeavour to point out generally and in some cases particularly how these diseases are to be dealt with, and in doing this we must view the question from the following points:—

1. The modification of the nature of contagium.

2. The destruction of contagium—*a*. Without the body; *b*. Within the body.

3. To counteract the effects of contagium.

1. But little way has yet been made towards the modification of the nature of contagium. In fact, the only contagium which we have yet been able to modify is that of small-pox, and our power to modify that contagium has reached an amount of exactness and certainty which affords the strongest proof of the value of experimental pathology, and the greatest encouragement to follow up those investigations (which are now becoming so common) into the nature of infectious diseases among animals, and their relation to human complaints. The results of vaccination are so well known, and the benefits it has conferred on mankind so extensive, that it is quite unnecessary for me to refer to them further.

2. The means at our disposal for the destruction of contagium are numerous and various. They differ so much in their mode of application, and the circumstances under which each may be used are so unlike that we are frequently unable effectually to apply any of them.

The agents termed disinfectants which are used for the destruction of contagium, must not be confounded with antiseptics, which prevent septic fermentation by the destruction of microzymes, &c. There is a strong *probability* that an antiseptic will prove a disinfectant, but that is all; it is not necessarily so, nor *vice versâ*. I have shown already that the power of some antiseptics is diminished by the presence of albuminous compounds, and as contagious particles are frequently, some say always, associated with albuminous substances, the power of these antiseptics must be thus diminished. Contagium within the system must of necessity be associated with albuminoid substances, and therefore possess additional protection against the action of antiseptics.

I shall now consider the action of disinfectants on contagium, and here again we must be chiefly guided by the results of Dr. Baxter's experiments. Dr. Baxter adopts as a definition of

disinfectant "any agent capable of so modifying the contagium of a communicable disease, during its transit from a sick to a healthy individual, as to deprive it of its specific power of infecting the latter." It will be observed that this definition excludes the destruction of contagium within the body. The particular forms of contagium selected for experiment by Dr. Baxter were vaccine, the virus of infective inflammation, and the virus of glanders. The disinfectants tested were potassic permanganate, sulphur dioxide, chlorine, and carbolic acid, the same used in his experiments on septic fermentation; also exposure to heat. The results of the experiments prove that—

a. Carbolic acid, sulphur dioxide, potassic permanganate, and chlorine, are all endowed with disinfectant properties.

b. The effectual disinfectant operation of chlorine and potassic permanganate depends more on the nature of the medium in which the contagious particles are contained than upon the nature of the particles themselves; thus, when the contagious particles are protected by the presence of organic compounds, or when agents capable of combining with chlorine or decomposing the permanganate are present, the powers of these disinfectants are so impaired that their action cannot be relied upon.

c. A virulent liquid cannot be regarded as certainly and completely disinfected by sulphur dioxide, unless it has been rendered permanently and strongly acid.

d. No liquid can be considered disinfected by carbolic acid, unless it contain 2 per cent. by weight of the acid.

e. "When disinfectants are mixed with a liquid, it is important to be sure that they are thoroughly incorporated with it; that no solid matters capable of shielding contagium from immediate contact with its destroyer be overlooked.

f. "Aerial disinfection, as commonly practised in the sick room, is either useless or positively objectionable, owing to the false sense of security it is calculated to produce. To make the air of a room smell strongly of carbolic acid, by scattering carbolic powder about the floor, or of chlorine, by placing a tray of chloride of lime in a corner, is, so far as the destruction of specific contagia is concerned, an utterly futile proceeding. When aerial disinfection is resorted to, the probability that the virulent particles are shielded by an envelope of dried albuminous matter should always be held before the mind. Chlorine and sulphur dioxide are, both of them, suitable agents for the purpose; the latter seems to be the more effectual of the two. The use of carbolic vapour should be abandoned, owing to the relative feebleness and uncertainty of its action.

g. "Dry heat, when it can be applied, is probably the most efficient of all disinfectants. But, in the first place, we must be

sure that the desired temperature is actually reached by every particle of matter included in the heated space; secondly, length of exposure and degree of heat should be regarded as mutually compensatory factors within certain limits. The above statements are not so discouraging as they may appear at the first glance to our reliance upon artificial disinfection. If we believe that all contagia are generated, like those of small-pox and scarlet fever, in the infected organism, and there only, the outlook is a hopeful one. We might even anticipate an approach to the *perfect* fulfilment of the work of disinfection, by subjecting all matters immediately after their removal from the affected person, and before any dilution or admixture, to the full influence of one or other among the destructive agencies at our command. On the other hand, if the contagium of any disease is capable of being generated *de novo* outside the body (pythogenic origin of enteric fever, typhus created by overcrowding), such contagium can hardly be eradicated by any method of artificial disinfection. For cases of the latter kind the opening words of the memorandum previously referred to furnish the only solution—namely, it is to cleanliness, ventilation, and drainage, and the use of perfectly pure drinking water, that populations ought mainly to look for safety against nuisance and infection. Artificial disinfectants cannot properly supply the place of these essentials, for, except in a small and peculiar class of cases, they are of temporary and imperfect usefulness."

Now, having stated so much with regard to the means we possess of destroying contagion *outside* the body, the next point to be considered is — what power have we of destroying contagion within the body? This is a question beset with many difficulties. It is quite clear that if we are to rely on disinfectants, these must be introduced into the system in sufficient quantity to cause the death of the living particles of contagion; and in doing this we must bear in mind that we do not cause the death of our patient by poisoning him with our remedies, or that we do not seriously damage or destroy some of the delicate organisms which are necessary to his healthy existence. Dr. Baxter and others have shown that the presence of albumen or bodies having a great chemical affinity for chlorine or oxygen prevent or counteract the effect of disinfectants, such as chlorine and potassic permanganate, and in a less degree sulphur dioxide. We cannot, therefore, hope that any benefit would accrue if we were able to introduce these bodies into the circulation in considerable quantities. In addition to this, we have to consider that, in the case of carbolic acid, which seems to be a powerful disinfectant, not much, if at all, impaired in its powers by the presence of organic compounds, the amount, 2

per cent., required to produce disinfection, is too great to be introduced into the system. It is known that if carbolic acid is applied over a very extensive raw surface, it may produce poisonous effects. I have myself considered it necessary to be very careful in the use of carbolic acid as a local application in small-pox on this account. We dare not, if we could, introduce 2 per cent. of carbolic acid into the circulation of a human being. Fortunately we are possessed of other disinfectants whose powers are not easily impaired by the presence of organic matter; these are quinine, salicylic acid, and *possibly* also thymol, carvol, eucalyptol, and other recently investigated antiseptics and disinfectants. The power of these agents to destroy contagion has not yet been properly tested, but estimamating their disinfectant by their known antiseptic powers, we have good reason to conclude that they are disinfectants. Quinine and salicylic acid possess this advantage, that they can be introduced into the system in large quantities without injury to the patient. We have, therefore, two drugs which have the power of acting as disinfectants within the system. This power, indeed, seems to have been to a certain extent demonstrated in the case of these drugs. We know that the effect of the action of any contagion within the system is to produce fever, and that the essential symptom of fever is a high temperature. We know that quinine and salicylic acid, administered in large doses, reduce temperature—in fact, suspend for a time the febrile processes, and the continued use of these drugs reduces fever. We know further from the experiments of Professor Binz, of Bonn, that quinine possesses the power of diminishing and, in the proportion of 1 in 4,000, absolutely destroying the motive power of white blood-corpuscles, or, as Dr. Beale would term them, the bioplasts or bioplasm of the blood. Without absolutely adopting Beale's views, I think he has offered strong evidence in proof of the view that all particles of contagium are bioplasm which has undergone certain modifications—in fact, he has to some extent shown it in the cases of variola and vaccinia. We may, therefore, fairly draw the inference that an agent which has a powerful action on the bioplasm of the blood or bacteria, and which produces results (namely, the reduction of fever) which would follow if it acted on the bioplasm of contagium within the system, has the power of acting as a disinfectant within the system. Such an agent is quinine, and to it and similar agents we must look for the power to control contagion within the body. The observations of Dr. Lapper, to which I have referred, point to the fact that certain drugs, when mixed with dead blood (in such proportions as could be introduced into living blood without danger to the patient), have the power of retarding decomposition. These

experiments are corroborative of the view that we do possess disinfecting agents capable of acting within the system. I think, therefore, we may fairly conclude that we can not only destroy contagion *without* but also within the body. The consideration of the counteraction of the effects of contagion would lead us into the discussion of the treatment of all the abnormal conditions that arise as a result of febrile processes set going by the growth of contagion within the system; but in such limited time it is impossible to follow all the details of so large a question. However, I will refer to a few. In Dr. Sanderson's Report on the Processes of Fever, he says:—

"A satisfactory explanation of the nature of fever and of its relation to the febrile process is not at present possible, because we are not as yet possessed of the necessary physiological knowledge. We have elsewhere stated that two possibilities are open to us. One is that fever originates in disorder of the nervous centres; that, by means of the influence of the nervous system on the systemic functions, the liberation of heat at the surface of the body is controlled or restrained, so that by retention the temperature rises, and finally that the increased temperature so produced acts on the living substance of the body so as to disorder its nutrition; the other alternative is that fever originates in the living tissues; that it is from first to last a disorder of protoplasm, and that all the systemic disturbances are secondary.

"In both hypotheses it is tacitly assumed that fever is the product of a material fever-producing cause, contained in the blood or tissue juice, the morbific action of which on the organism is antecedent to all functional disturbances whatever. At bottom we are all humoralists, and believe in infection. It is not until we have to say when and how the infection acts that questions arise. The facts and considerations we have had before us are, I think, sufficient to justify the definitive rejection of the first hypothesis in all its forms; for, on the one hand, we have seen that no disorder of the systemic functions or of the nervous centres which preside over them is capable of inducing a state which can be identified with febrile pyrexia; and, on the other, that it is possible for such a state to originate and persist in the organism after the influence of the central nervous system has been withdrawn from the tissues by the severance of the spinal cord. We are, therefore, at liberty to adopt the tissue origin of fever as the basis on which we hope *eventually* to construct an explanation of the process."

As variations in temperature in fever seem to measure more accurately than anything else the variations in the condition of a patient suffering from fever, the effects of various treatments on febrile temperature have lately excited special attention.

Three plans for the reduction of temperature seem to have been pursued:—

1. The natural one of application of cold (the cold bath, &c.)

2. The administration of aconite and similar agents, which reduce the force and rapidity of the circulation.

3. The administration of quinine, &c., which tends to diminish the febrile processes by counteracting the growth of contagium.

It must be admitted that the excess of heat produced during fever is the result of the chemical process going on within the body, and that these chemico-vital phenomena are the result of the growth of the contagium, which will continue until all suitable soil is exhausted. Taking this view of our question, it scarcely seems reasonable to expect that the mere abstraction of heat will in any way retard the processes going on. It is somewhat like attempting permanently to reduce the temperature of a room, over-heated by too large a fire, by occasional opening of the windows. No sensible person would make the attempt, but at once reduce the fire. The opening of the windows would make the room more comfortable for its occupants for the time being, just as cold water may make a fever patient more comfortable, but the cause of the fever is not influenced. I have tried (as I felt bound to do) the use of cold water in the treatment of fever, but must confess I found it useless in all, and even injurious in some cases.

The grounds for the employment of such drugs as aconite for the reduction of temperature seem to be insufficient. They lessen the rate of the circulation, and, therefore, reduce the amount of heat carried by the blood to the surface in a given time, and, in consequence, the sensible amount given off; but does not this rather tend to prolong the *fever* than to diminish it? If the heat is produced by tissue change it must be given off, and aconite has not been found to shorten fevers. Aconite is a powerful depressor of the circulation, and its use on this ground alone is contra-indicated in fever.

The grounds for the use of quinine and similar drugs are much stronger, as they absolutely tend to *diminish* the febrile processes, and not merely remove the *result* of the processes. It is in the study of the antipyretic drugs of this class that the greatest triumphs may be expected. It is among these that we must look for the power of cutting short a fever, as rheumatic fever can now be cut short by the use of salicylic acid. The doctrine that a fever cannot be cut short must be given up by physicians, and with it, of course, the so-called "expectant treatment of disease." But another question still arises. The most dangerous specific contagious diseases, as a rule, attack

individuals only once in their lifetime, and the accepted explanation of this is that the first crop of the contagium exhausted the soil necessary for its growth, and until a new soil be produced the same disease will not again take root. Now, supposing we cut short the progress of a contagious fever by killing its germs within the system before the soil is exhausted, will not the soil so left render the patient liable to a future attack of same disease? This is a question yet to be answered. —*Dublin Journal of Medical Science, July and August*, 1878, *pp.* 1, 96.

2.—ON THE PATHOLOGY OF FEVER.

By Dr. HENRY F. A. GOODRIDGE, Senior Physician to the Royal United Hospital, Bath.

[The following is from the Address in Medicine at the late meeting of the British Medical Association at Bath.]

The ultimate causes of fever, we have come to learn, lie for the most part outside the body. A *materies morbi*, a pyrogenic virus, is taken into it; how, is not sufficiently known; but in various ways we can, in the warm-blooded animal, artificially produce fever. As to the nature of this virus, we need not to be reminded that recent pathological researches have wonderfully enlightened us. We now know that the virulent matter is not in itself liquid, but particulate, composed of corpuscular particles; that it is not matter possessed of peculiar physical or chemical qualities, but that, in many cases at least, it consists of distinct organisms or their germs, capable of development and endowed with the property of self-multiplication. Only in one or two diseases in man, I believe, have specific organisms as yet been actually demonstrated, but the existence of such in others is capable of indirect proof, and in others still is an almost imperative assumption or a more or less probable supposition. Indeed, if we are prepared to go along with an eminent German pathologist, we shall bring into the category of diseases caused by parasitic organisms, not only those just referred to, but nearly all febrile diseases, acute rheumatism, and rheumatic endocarditis, pneumonia, nephritis, tuberculosis, syphilis, &c.; nay, this authority anticipates the time when all infective diseases—among which, of course, he includes those enumerated—will be classified according to the botanical genus and species of the organisms which have occasioned them.

But to return to the pyrogenic virus in its relation to the proximate causation, the mechanism, of fever. Where the virulent particles, the organisms or germs, are deposited until according to the duration of the incubation in each instance they get into the blood, is, except perhaps in the case of one or

two diseases, unknown. That they *do* get into the blood before exerting their special activity, seems admitted on all hands. Their arrival in the capillary vessels was pretty conclusively shown by Chauveau's "bistournage" experiments. That these virulent particles, moreover, or, if not themselves, secondary particles developed under their influence, products of decomposition, thrown into the periphery of the vascular system, make their way through the walls of the capillaries and diffuse themselves in the tissues, may perhaps be an assumption not unwarranted by the present state of our knowledge. But here, I need not say, we reach its limits. Much prejudice has arisen in some quarters against speculative views in matters of science, but in my individual opinion, those of original investigators are entitled to attentive consideration, as at least indicating future paths of inquiry. Buss's theory, then, briefly is as follows. According to him, these particles, escaped from the vessels and free in the tissues, exercise an irritating influence upon the terminations of the sensory or afferent nerves: they gradually again are taken up by the lymphatics, and, returned to the venous blood-stream, circulate afresh, reach the periphery, and so on. Thus there occurs a gradual saturation of the entire tissues of the body with these irritating particles, and a permanent irritation of the entire sensory nerve-terminations is kept up. This will tell upon both the sympathetic and the cerebro-spinal afferent nerves. In the former case, constriction of vessels will ensue; in the latter will develop, in a reflex way, in like manner as in the normal regulation of heat, an increased passive (this word being allowed) or even active muscular activity, and therewith a similarly increased production of carbonic acid. Accordingly, there will be persistently maintained a condition of more or less exaggerated contraction of the peripheral vessels, as also of increased production of heat. This excessive vascular contraction necessarily reduces in proportion the heat-discharge. Also the heat-production is no longer influenced alone by the stimulus of cold, but simultaneously by the irritating particles also. Thus the temperature of the body reaches a definite abnormal height, determined by the intensity of operation of the irritating particles, and will remain at the same so long as the irritating operation continues at the same amount. The regulation of heat, moreover, is set accordingly.

It will be seen, then, that by this theory the kind of influence of cold, and of these pyrogenic particles upon the nervous system, is exactly the same. Both effect in the same way peripheral contraction of vessels, and excite the latent muscular activity. But there is yet an essential difference. The operation of the stimulus of cold is, *eo ipso*, always connected with

abstraction of heat, a peculiarity which is excluded in the operation of irritating particles. And as the regulation of heat of the healthy person is adapted to the stimulus of cold—that is, to a stimulus attended with simultaneous loss of heat—and can regulate exactly only for this, it answers necessarily to the irritation arising from the pyrogenic particles in like manner as if it were the stimulus of cold. And as with it no abstraction of heat stands in connection, it follows that the temperature of the body must be raised above the normal. Herein, then, according to this authority, lies the true cause of the febrile elevation of temperature, and the essential reason why, with the most heterogeneous causes, always one and the same effect ensues, viz., the febrile elevation of temperature.

Samuel's views on the pathology of fever are more readily told. While recognising generally the mechanism of the heat-regulation, before indicated, he holds that certain matters in the blood of a chemical nature, products of tissue-change, concur with the thermal influences (stimulus of cold) in maintaining the reflex ganglionic centres, which he calls "heat centres," and considers automatic, in their normal persistent excitement; and that the essence of fever lies in an excessive production of these matters (Ganglienreize), which is followed by that of similar matters, supposed by him to be the special stimuli of other automatic centres, and thus is determined an overaction, an abnormal tension, of a group of centres. Changes in the blood, then, Samuel regards as the essential *point de départ* of the febrile disorder, whether these changes be called forth by the introduction of foreign substances from without or not. He enunciates his theory thus: "Fever depends upon a simultaneously exalted tension of the automatic heat, and heart-centres, and upon an intercurrent one of the automatic vaso-motor nerve-centres, and of the ganglia of the sensorium; that this tension, however, is brought about not directly by introduction of pathological nerve-irritants, but indirectly by increased production of the physiological nerve stimuli in the blood." Professor Wood of Philadelphia, U.S., has recently carried out a series of experiments in the dog, on the effects of section of the spinal cord upon the evolution and production of animal heat; but, as far as I can gather from a brief report of the same, with no very novel results. He found, as others have done, that at a low external temperature, after section of the cervical cord, there was increased evolution, with diminished production of heat; while, at a high external temperature, both the production and evolution of heat were diminished. The animal died in winter of cold, and in summer lived much longer on account of the high external heat. Wood attributes the fall of temperature, as likewise others have

done, to paralysis of the vaso-motor nerves of the arterioles at the surface removing the restraint upon heat-loss, and this, of course, is one factor in the result. But the main one is clearly, as Samuel's experiments show, the paralysis of the muscular nerves, and the consequent suppression of the heat-producing processes of the musculature. Here may be the proper place to observe that, neither from these experiments, nor from any other source, is a quite satisfactory explanation yet forthcoming of those rare, but still well-authenticated, cases in which, after accidental injuries of nervous centres in man, or artificial lesions of the same in animals, remarkably high temperatures have occurred and with a cool surrounding. The possible existence, however, of an inhibitory centre, perhaps in or near the upper part of the cervical cord, has been before duly recognised.

Leaving to others more competent the task of criticism, I may yet be permitted a word or two. It appears to me, then, that Buss has conclusively shown that a *relatively* diminished heat-discharge is an essential constituent of the febrile condition. That the pyrogenic particles, or "irritaments," as he calls them, should select the sensory nerve-endings for their special operation, if ultimately proved to be correct, must at any rate, for the present, be taken as pure hypothesis, and, as far as I know, unsupported by analogy. Samuel seems to regard increased heat-production as predominant in the genesis of fever—this being, as we have just seen, the result of an excessive production of his also hypothetical ganglion-stimuli. He, however, *does* appeal to analogy, citing an observation of Ranke's in the frog, viz., that urea and hippuric acid exercise a direct influence upon Setschenow's reflex inhibitory centre. In the higher animals also, as is well known, the respiratory centre is affected by the condition of the blood; but the analogy hardly serves further, for it has been found that it is not the excess of carbonic acid which affects the centre, but the deficiency of oxygen.

As was intimated at the outset, there are some authorities who do not admit the dependence of fever upon alterations of the nervous system, but contend that it originates in the living tissues, and is from first to last a disorder of protoplasm. This is the view of our eminent English pathologist Burdon Sanderson, as maintained in his elaborate article in the Local Government Scientific Reports of 1875. His argument seemed based very principally upon certain experiments of Murri, a Florentine physician, made upon the dog. Having first divided the cervical cord (which produced a cooling), Murri injected fetid pus under the skin; whereupon, he affirms, the temperature rose quite as much as after injection of the same in animals not operated on. But these experiments, as detailed in the original Italian publi-

cation, have been carefully examined by Handfield Jones, and he considers the results do not warrant the statement made. "Though some elevation of temperature followed the injection of pus," says the latter able physician, "yet it never reached fever-height in rectum or skin, and soon fell again considerably." And, in reference to another experiment, "it is clear that the pyrogenic effect of the pus did not avail to bring up the temperature to the normal, and consequently the result was not such as in animals with sound cords." Three years and more have elapsed, and one has heard of no repetition of these experiments or other evidence confirmatory of Murri's statement. On the contrary, Samuel's experiments prove, as we have seen, that alike the febrile heat and the normal heat are dependent upon the musculature, and that when this is paralysed, as in section of the cervical cord, the heat of the body is dependent upon that of the surrounding medium. Moreover, the fact remains that no one yet has ever succeeded in producing fever in cold blooded-animals, to whose thermal condition the warm-blooded one is reduced by section of its cervical cord.

Very interesting are the recent observations of Volkmann and Genzmer on the aseptic wound-fever. Its apparent independence of all external infection (as in the case of subcutaneous injuries), the presence of high temperature (even to 40 deg. C. —104 deg. F.) with absence of the ordinary symptoms of disorder, and its harmless course and issue, are remarkable peculiarities. The observers themselves attribute it to absorption of matters nearly allied to the physiological products of retrograde tissue-metamorphosis, instead of to heterologous matters, as in septic fever—an explanation quite in accordance with Samuel's views, but admitted by them to be provisional only. We have yet to learn more about aseptic fever. In reference, however, to the tolerance of high temperature, I must confess myself to be one of those who hold to the doctrine of Liebermeister, that unremitting high temperature constitutes the chief element of danger of fevers generally. While he was the first to insist on this point, practical medicine is not less indebted to him for introducing the antipyretic treatment by which this danger may be in great measure controlled.

A word upon hyperpyrexia. This, as we know, mostly occurs in the course of acute rheumatism or rheumatic fever, and it has long been my conviction that in a majority of cases the same is due to mismanagement. What determines the remarkable activity of the sweat-glands observed in this disease is not yet ascertained; but a very little consideration suffices to show that in any given case, except for this copious perspiration, the temperature would at once be higher; it is just so much natural heat-

discharge; and the true indication would be to promote it and assist cutaneous transpiration. But how often is just an opposite course followed! The patient, from an exaggerated dread of chill and the painfulness of all movement, is allowed to remain for days together under an impervious load of blankets and counterpane, enveloped perhaps in flannel, his skin uncleansed, reeking in his own exhalations, and with, by-and-by, a copious miliary eruption testifying to the folly perpetrated. Without ignoring individual susceptibilities, I believe it is under these circumstances that rheumatic hyperpyrexia is most prone to arise. A case of my own, some years ago, in which the employment of warm baths (not cold) obviously contributed to recovery, first suggested to me this view, and further experience has only served to confirm it.—*British Med. Journal*, *Aug.* 10, 1878, *p.* 202.

3.—REMARKS ON THE ORIGIN OF INFECTION.

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(From a paper read before the Epidemiological Society.)

Can any case of acute specific fever arise independently of an antecedent case? And by the term acute specific fever, I mean all acute febrile diseases capable of reproducing their own kind. With a view to the elucidation of this question I will in the first instance attempt a review of the principal grounds which might lead some observers, on the one hand, to answer it in the negative, others, on the other hand, to reply affirmatively.

I. The following arguments might be brought forward by those who, at times termed contagionists, maintain that no case of infectious disease can arise independently of a prior case of the same disease.

1. There is the general inference to be drawn from the well known fact that infectious diseases do largely owe their spread to communication, immediate or mediate, with prior cases of the same disease. With regard to many of these diseases we know what are at least the ordinary means by which their poisons are eliminated from the bodies of those affected by them, and these poisons we can trace, at times through numerous and intricate channels, until at last we find that in some way or other they are received into the systems of some hitherto healthy persons, in whom forthwith the diseases they give rise to are reproduced with all their well known signs and symptoms. This then being admittedly the ordinary source of infection, surely, it may be urged, there are strong *à priori* grounds for assuming it to be the invariable one; for even when direct evidence as to the transmission of the specific poison cannot be traced, there must be ample reason for assuming that in some hitherto undetected

way the same process has been followed out. Indeed the alternative view is, it may be alleged, founded solely on negative evidence, and is based on nothing better than the failure of the investigator to trace out a succession of circumstances, often obscure, but which are well known to have existed in many other similar instances.

2. And this first argument is strengthened by consideration of the fact that new channels by which the poisons of these specific diseases can be conveyed from one person to another are periodically being discovered; and that consequently outbreaks, which formerly would have been brought forward as affording indubitable evidence as to spontaneous origin, on the ground that all known sources of the diseases in question could with certainty be excluded from the list of possible causes, are now known to have been due to the conveyance of the poison by means of some hitherto unknown channel. Thus since it was ascertained that enteric fever and scarlet fever could be transmitted through the agency of milk as a vehicle, numerous outbreaks of these diseases, especially of enteric fever, have been ascertained to have owed their spread to this means, and thus that which must formerly have remained unexplained on the theory of the contagionist, now tends to confirm his views. The same remark applies to the striking outbreaks of enteric fever which have owed their spread to that form of water-pollution which in intermittent water-services leads to the specific poison of the disease being drawn with other filth into the water-mains during periods of intermission, and to its being subsequently distributed to the general body of consumers. And if milk can thus act as a vehicle for specific infection, and if water can thus in this newly-discovered method transmit the poison of enteric fever, may not other articles of food act in a similar manner; and may not water be a carrier of infection by other and, as yet, unknown means? In short, is it not likely that other channels for the communication of infection may be discovered, and that in this way all doubt as to the possibility of the spontaneous origin of the diseases under consideration may, step by step, be removed? And hence, where all known methods for the conveyance of these poisons fail to explain the existence of the diseases produced by them, is it not fair to assume that the infection has really been communicated, though by some hitherto undetected channel?

3. In cases, too, where the history of an attack of infectious fever appears to afford strong evidence in favour of its being due to spontaneous origin, it often happens that, as the result of renewed and skilled investigation, or owing to the accidental discovery of some apparently trivial circumstance, the disease is after all found to be due to infection, from some prior case.

Many such instances must have been noted by the members of this Society. One which came under my own notice I will record. A woman living in a village which had long been free from enteric fever, sickened with that disease, and, after having communicated it to others, she died. The previous history of her attack had, up to a certain point, been somewhat carefully inquired into, and it was regarded as an instance of enteric fever of pythogenic origin. At a date prior to this woman's illness, which fairly represented the ordinary incubation period of enteric fever, she had started on a holiday trip, and at a certain railway junction the train she was in had been shunted on to a siding so as to bring the carriage she was in into close contact with some trucks laden with highly offensive, but otherwise ordinary farmyard manure, which was exposed to the full rays of a midday sun. The effluvia arising from this fœtid refuse was such as completely to overpower the woman; indeed, she felt quite unable to continue her journey, and at the first opportunity she quitted the train and returned to her home. She was stated never to have "got over" the smell of this manure, which clung as it were to her nostrils, and after a feeling of general malaise and discomfort lasting from eight to ten days, she sickened with well-marked enteric fever. So far there seemed strong grounds for believing that the disease was really brought about by the emanations in question, and that the case afforded proof that enteric fever is not invariably a self-propagating disease. It was, however, subsequently ascertained that just prior to her intended holiday trip, and also well within the incubation period of enteric fever, the woman had been to a neighbouring town, and inquiry there elicited the fact that she had stayed at a house where a fatal case of enteric fever had occurred, and that she had there been exposed to the very conditions which had led to the outbreak in the house she visited.

4. Another point which may be urged by the contagionist is, the fact that periods of incubation, known by all to be subject to certain variations in length, do in exceptional instances vary much more than is generally recognised, and in consequence constitute a source of error for which due allowance is not made. Dr. Budd in his work on Typhoid Fever, refers to cases in which the incubation period of that disease could not have exceeded four, and even two days; indeed so rapidly did the disease in some instances supervene on exposure to infection, that it was at first looked upon as the result of some chemical poison. In at least one group of cases the proportion attacked out of those exposed to the infection was so large as to make it practically certain that the disease was in reality the result of the previous exposure, and Dr. Budd regards the unusually short period of incubation as affording evidence of a high degree of

concentration of the poison received. I have myself met with instances in which the facts elicited appeared to afford evidence of exceptionally prolonged periods of incubation. In one instance I found that a young woman, residing in a hamlet which had certainly for several years been free from enteric fever, was attacked with well-marked symptoms of that disease. Nearly a month prior to her attack she had returned from a visit to a distant part of the country, where there are strong grounds for assuming that she came into contact with the infection; and it is certain that after her return home she had neither left the hamlet, nor had any person from elsewhere visited her house. In short there must have been, according to this view, a period of incubation all but a month in length.

In this case there was, however, a peculiarity worth noting. Just within fourteen days of her return, or in other words, just about the time when the early symptoms of enteric fever ought to have manifested themselves, this young woman suffered from a sharp attack of diarrhœa, which, after lasting a day or two, left her apparently in perfect health, until after a further lapse of twelve days, when unmistakable symptoms of enteric fever came on. In this instance it is just possible that the attack of diarrhœa, which occurred nearly a fortnight before the ordinary symptoms of enteric fever commenced, might have been a part of the disease, and that a partial elimination of the poison was then effected. If this be so, however, we are only presented with another point of difficulty in connection with the etiology of enteric fever, which those who believe that the disease is a solely self-propagating one are entitled to claim, as affording an additional argument in favour of their view.

5. And again, there are facts which are open to the interpretation that, in certain cases where strict investigation fails to elicit any information as to the possibility of recent infection, an outbreak of some acute specific fever, instead of being due to spontaneous origin, is in reality the result of infection from some prior case of the same disease which had occurred at some distant date; the specific poison having remained dormant, and having retained its vitality for a long period of time. That some "organisms," especially those of vegetable origin, do at times retain their vitality for long periods whilst in an apparently dormant state has long admitted of no doubt. And it is now further known that the "organism" found in one of the acute specific fevers affecting the lower animals, namely splenic fever, is capable of thus retaining its vitality. It exists in at least two states, in one of which it is liable to undergo rapid development; in the other it is permanent and resistant. Some of these "spores," according to Dr. William Roberts, F.R.S., retain "their infective activity for an indefinite period in spite

of all kinds of maltreatment." They can "be reduced to dust, wetted and dried repeatedly, kept in putrefying liquids for weeks, and yet at the end of four years still display an undiminished virulence." In fact, we know no reason why the infection of the various acute specific fevers should not last, under certain circumstances, for an indefinite period, much in the same way as grains of wheat have in exceptional instances lain dormant for some thousands of years, and then borne fruit. With regard to scarlet fever, there are several instances on record which tend to show that its poison will remain dormant for more than a year, and then act with unabated virulence. Instances have come under my own notice, in which the facts warranted the conclusion that the poison of diphtheria had been retained for months about premises in which cases of this disease had previously occurred. Dr. A. Carpenter, in a paper read before the British Medical Association in 1875, records a case in which the poison of small-pox had evidently been retained in a so-called dormant state for a long period. "He remembered a practitioner in Oban informing him of such a case occurring in Mull. A proprietor of that island ordered the removal of some old houses which had lain in a state of ruin and uninhabited for many years. Eight men were employed in digging the walls, and every one was attacked with small-pox; the germs of that disease," to use the writer's own words, "having lain in these houses all those years."

Similar evidence with regard to enteric fever has often been recorded. Dr. W. Ogle, in some of his reports on the sanitary condition of the East Herts district, refers to cases in which attacks of this disease have in successive autumns occurred in isolated dwellings, where, so far as could be ascertained by the strictest investigation, no new source of infection could have come into operation. Dr. Budd writes, "I once knew a labourer's cottage which remained vacant nearly two years, in consequence of nearly every inmate of it having contracted typhoid fever. At the end of that time it was retenanted, and three weeks had scarcely elapsed before several of the new inmates were simultaneously seized with the same fever. The cottage stood alone in a secluded spot, and there was no fever in the neighbourhood at the time of the second attack." I know a detached house which stands in large grounds in a country district, and which was occupied by a groom and his family, amongst whom enteric fever prevailed in the autumn of 1872, one case being fatal. This family continued to occupy the house for nearly two years after this occurrence, but they left it some time in 1874 in consequence of the departure of the owner of the estate on which it stood. From that date the house remained unoccupied until February, 1876, when it

was tenanted by new inmates; and exactly within fourteen days of these latter taking up their abode there enteric fever broke out amongst them, and a most careful inquiry led both the medical man in attendance and myself to the conclusion that the disease was not imported. I also know an isolated parsonage in which the families of two successive vicars have been attacked with enteric fever.

One more instance, which is recorded by Dr. Gilbert Child, I would refer to, because of the evidence it contains as to the communication of enteric fever, after a lapse of considerable time, by means of fomites. On the 20th of November, 1873, a man arrived at a house in an Oxfordshire village, where his mother and his brother lived, bringing with him several boxes of some very filthy bedding, clothes, &c. On the 29th of the same month the brother was seized with a fatal attack of enteric fever, and on the following day the mother fell sick of the same disease. No source of infection being known, the man referred to was questioned as to his prior movements, and it transpired that in the previous September his wife had died of enteric fever at Toronto, and that he, on subsequently leaving America, had brought away with him her dirty bedding and clothing. The only other case of enteric fever which could be heard of anywhere within the district was that of a woman who was attacked on the 5th of December, and she, being also one of the man's relations, had taken to her home some of the dirty things to wash.

Instances such as these, it may well be alleged, strongly support the view that the poisons of the acute specific fevers do, under certain circumstances, retain their vitality for very long periods of time; or in other words, that they "possess the wonderful property of passing into a state of persistent inactivity or latent vitality, in which they perform no function, but can at any moment, be wakened up into active function, whenever they are brought under favourable circumstances."

6. That the diseases under consideration are purely self-propagating may also be urged in view of the long-continued and complete immunity from them which certain isolated countries have enjoyed, and of the virulence which such diseases have exhibited when once introduced into them. Sir Thomas Watson, in his somewhat recent article in the 'Nineteenth Century,' recalls the circumstance that the New World was free from small-pox prior to its discovery by Columbus in 1492, and in the same way this disease was unknown in Mexico, Iceland, and Greenland until its importation into them, when it spread with terrible desolation among their populations. We have the same experience with regard to the long immunity which the Faroe Islands and Fiji enjoyed from measles, which disease, on being

imported, spread through them with all but unparalleled fatality; and a similar long-continued absence of these diseases has been noticed even when the conditions with which their spread is generally associated, have been rife. Thus conditions favouring excremental poisoning of air breathed, or of water drunk, are known to have existed for years without producing a single case of enteric fever; and yet when the disease has been imported the violence of its spread has shown conclusively how suitable a soil for the propagation of the poison existed ready to hand.

These, besides other arguments which will doubtless present themselves to your minds, may with justice be used by the supporters of the theory that the acute specific fevers never arise independently of a prior case, and they undoubtedly constitute a somewhat formidable array, the more so when it is remembered how almost impossible it is for any one holding opposite views to state with confidence, that as regards any special case, all such sources of error can with certainty be excluded.

II. There are however observers, and amongst them men of great distinction, who confidently hold the opposite view, and whilst freely admitting that the diseases in question are self-propagating, yet unhesitatingly assert that some of them at least do at times arise independently of antecedent cases; that they are in fact of spontaneous origin.

As a mere matter of argument, the view they hold is by no means so easy of support as that of those who declare that these diseases are solely and exclusively self-propagating; for whereas the latter can in numberless instances bring forward positive evidence in favour of their conclusions, the former have the far more difficult task of proving a negative; and the mere enumeration of all the sources of error which must be avoided before any positive conclusion can be arrived at in the matter, shows how laborious of proof the position they have taken up becomes. Those, however, who do believe in the independent origin of these poisons have important points to urge in behalf of their views.

1. Judging from analogy, they may point out how their opinions derive support from the well-known fact that "spontaneous generation of the traumatic infections" does "take place," and that their respective contagia do arise in putrefactive wound products. As the result of somewhat recent experiments, Dr. Burdon Sanderson has found that "if the exudation of a simple peritonitis be injected fresh into the peritoneum of another animal, the disease assumes a more intense form in the second than in the first,"—and if in this way the disease be communicated to several animals, and by "artificial selection

the most severe case be picked out, and the exudation from that case be used for further transmission, a still more intense inflammation will be the result, until at least a virus is obtained of which the virulence resembles that of the specific cases of malignant peritonitis in the human subject." Here, it may be alleged, is a process in every way resembling the development by progressive stages of that which appears to be a specific poison, the starting-point being a simple inflammatory fluid exposed to a series of circumstances favourable to such development.

2. Then there is the fact that certain of the acute specific fevers are known to follow on a concurrence of certain well-known conditions, and this under circumstances which enable observers to assert that there is a total absence of anything tending to indicate the possibility of the diseases having been imported. The manner, for example, in which typhus attacks armies when they suffer from ill-feeding and overcrowding, especially when those conditions are associated with the want and misery and depressing emotions following on defeat. The outbreaks of this disease during the recent war in the East remind us forcibly of the conditions under which it is said to arise amongst large masses of ill-cared-for troops in localities where it cannot be supposed that any previous cases of the disease have existed. The terrible havoc which typhus made both amongst the besiegers and defenders of Sebastopol is another case in point.

3. And further, all careful observers must admit that not unfrequently, when cases of infectious fever break out under circumstances which appear to afford every facility for ascertaining in every detail the previous history of the persons attacked, yet no amount of the most careful and exhaustive enquiry can elicit any information justifying the assertion that the disease is due to some antecedent case. If such cases were of rare, or even of somewhat unfrequent occurrence, they might perhaps, with but little hesitation, be set aside. But no one having access to the reports of medical officers of health can fail to be struck with the number of instances in which trained observers, carrying on their investigations in isolated localities free from the ordinary sources of error attaching to life in large towns, and making full allowance for all known sources of error, entirely fail to connect outbreaks of some of these diseases, especially of diphtheria and enteric fever, with any antecedent case, whether near or remote; and hence these observers feel themselves compelled to fall back upon the view that such diseases do, at times, have an independent origin. My own failures to elicit any history of previous infection as regards cases of infectious inflammation of the throat—of diphtheria—

which have occurred in isolated houses lying in districts far removed from other human habitations, have forcibly struck me, the more so as in the majority of instances those first attacked have been children of such tender years that all their movements could be ascertained with remarkable accuracy.

4. And again with regard to instances like those which have already been referred to in the argument on the other side of the question—instances in which a disease such as enteric fever recurs in successive years, or after a lapse of years, in houses where it has previously existed—instances which are assumed to afford evidence of the poison being retained as it were in a dormant condition; it may be alleged that the conditions favourable to the development of the poisons being present, each separate outbreak is but the result of a recurrence of the process by which a new poison is each time brought into existence.

5. And yet once more, there is an argument which cannot be lightly passed over. Those who believe that the poisons of the acute specific fevers may and do arise spontaneously have a perfect right to remind their opponents that there must have been first cases of each of the diseases under consideration, and that unless we are prepared to accept the view that the several contagia, which breed truly by a "process as regular as that by which dog breeds dog, and cat, cat," were originally the result of definite acts of creation, it must be assumed that they in some other way did arise independently of antecedent cases; and what right, they may ask, have we for asserting that a process once possible is now impossible? Dr. Budd says an argument such as this constitutes "the last refuge of the partizans of spontaneous origin;" but if it be so, it is a stronghold in itself, and one which must at least be noticed before it is passed by.

Thus far I have endeavoured, and I trust successfully, to take neither side in the argument, and were it not for the circumstance that the "Remarks" I have so far offered would have had an appearance of incompleteness if I did not in some way indicate how I was influenced by the points brought forward, I should have preferred at once to draw to a conclusion. And if I do proceed briefly to explain the effect which some consideration of the subject has had upon my mind, it is on the understanding that I deem the matter as one still *sub judice*, and that I am unprepared to offer anything which I can look upon as a final opinion in regard to it.

There do however appear to me to be some grounds for doubting whether certain of these diseases, as at present met with, are exclusively self-propagating, and for raising the question whether they may not occasionally arise independently of ante-

cedent cases. Such personal experience as has inclined me towards this view is mainly limited to investigations into the origin of outbreaks of diphtheria, and one point with which I have been specially struck is the following:—In isolated districts, and in houses situated at times miles away from other habitations, and in some instances lying in lonely spots among mountain ranges, where a visit to or a visit from the nearest town or village would be a circumstance too important to be forgotten, I have met with instances of what appeared to me to be nothing more than a simple inflammation of the throat, at times so trivial that it has passed all but unnoticed, and yet it has led by transmission through other persons to cases of well-marked and severe diphtheria. The first attacks have often happened in children whose former movements could apparently be traced with the strictest accuracy; they have occurred under circumstances which did not appear to admit of previous infection; and it has been difficult to interpret their occurrence except on the supposition that in some way they have arisen independently of prior cases. With regard also to the well-marked attacks of diphtheria to which they seemed to give rise, all other sources of infection could be excluded with a degree of certainty rarely to be met with. And as to those first affected, whose cases appeared to be earliest in a series which led up to attacks of well-marked diphtheria, it has more than once happened that they were children in whom so-called "sore-throat" was a common affection, and whose fauces when examined exhibited a loss of tissue indicative of former throat attacks.

I have stated that instances such as these have been met with in connection with outbreaks of diphtheria, and it may therefore be inferred that the early cases were merely mild attacks of that disease, the poison of which had been received in some unascertained method; but every one of these instances occurred in persons living miles away from the apparently very limited infected localities, which in some cases were mere hamlets, and this source of error seemed to be one which could unquestionably be excluded. And during the same investigations I have noticed the fact that over an area of some miles around the district in which genuine diphtheria was prevailing, there existed prior to as well as during the diphtheria epidemic numerous instances of sore throats which, so far as examination of the patients was concerned, in every way resembled the early cases above referred to, and yet which gave no indication of being infectious; sore throat being in fact in the surrounding district a prevailing ailment. And I have hardly been able to refrain from drawing the conclusion that conditions very similar to those under which genuine diphtheria was epidemic in a

limited district obtained, and had obtained before genuine diphtheria was anywhere seen, over a wide area around the immediately infected locality, and that these conditions, leading to a somewhat general predisposition to simple and apparently non-infectious inflammatory sore throat, had further, probably under somewhat modified circumstances, tended at certain points to produce an affection capable of putting on the property of infectiveness, which thus led to the transmission of the disease in a distinctly communicable form to others.

If this be a correct interpretation of the circumstances related, it would appear to indicate the possible occurrence of what may perhaps be looked upon as the progressive development of the property of infectiveness. And if the contagia of the acute specific diseases do, as has been suggested by more than one observer, belong to the vegetable world, I know no grounds for refusing to believe that organisms capable of producing a minor and an uncommunicable disease in particular stages of their growth, may in other stages of their growth, or in the course of their subsequent development, become capable of producing a major disease communicable from person to person; the affair being essentially one of soil. This is not at all a question of the development of a living organism out of matter independently of antecedent life, but merely the production by means of a process of evolution of that which gives to an already existing organism that property by which it becomes infective, a property which it may perhaps lose directly it is deprived of the circumstances which favoured its development in much the same way as special characteristics may be artificially developed in higher plant life, and be as easily lost again. —*Practitioner, June* 1878, *p.* 467.

4.—USE OF THE GRADUATED BATH IN HYPERPYREXIA.

By Dr. WILLIAM M. ORD, Physician to St. Thomas's Hospital.

The term "graduated bath" is here used to denote a bath in which the temperature of the water at the time of immersion is at some point between 90° and 100° Fahrenheit, and is gradually lowered by from 25° to 30° during an immersion of half-an-hour's duration. The term therefore does not include the cold bath of 55° or under which is sometimes used, but to which no recourse has been had, at least of late, in this hospital.

The Bath in Acute Rheumatism.—Attention may first be directed to the two cases of acute rheumatism in which the bath was used. They were cases of great severity, with extensive and very sharp inflammation of joints, and with endocarditis. Both cases were treated at first with salicylate of soda.

In the first, a woman of 35 years, the salicylate was begun

in doses of twenty grains every four hours. At the end of twenty-four hours, after six doses had been given, the dose was raised to thirty grains, the frequency increased to every three hours. While under this treatment the patient became more feverish, a profuse eruption of sudamina occurred, and noises in the head, and feelings of giddiness were complained of. At the eighth dose the salicylate was stopped, the temperature being 106·0°. The bath was then used, and the temperature at the end of the half hour's immersion, when the bath was at 70°, had fallen to 96·5°, a difference of 9·5° Fahr. This crushed the fever for the time and for many days, though a subsequent rise of temperature occurred and was subdued by the salicylate. It may be noted that the urine did not become albuminous in this case.

In the other case, the salicylate was given from the first in doses of thirty grains every two hours, and was stopped at the seventeenth dose. No nervous symptoms are recorded; but sudamina were abundant and resembled croton oil rash, while the urine was albuminous.

Recourse was had to the bath because the temperature had risen briskly during the administration of the salicylate, and after the seventeenth dose had reached the dangerous figure of 106·4°. The bath was graduated from 90° to 64° during half-an-hour, and at the end the patient's temperature was 96·6°, a fall of nearly 10° Fahr. The fever was completely subdued, normal temperature being permanently resumed on the fourth day after the bath; the patient was allowed to get up on the twelfth day, and was presented on the fifteenth day.

The fact of the complete failure of the salicylate though given in full and rapidly repeated doses is the first point of interest in these cases. If the doses be counted up, 360 grains of salicylate will be found to have been given in forty-eight hours in the first case, and 510 grains in thirty-six hours in the second. The toxic influence of the drug was well declared, by noises in the head and giddiness in the one, by albuminuria in the other. Nevertheless the temperature continued to rise in both instances to 106° or upwards. The effect of the bath was in both to produce an immediate fall of between 9° and 10° Fahr., and a complete cessation of pain, restlessness, and other symptoms attendant on fever. And the effect was also lasting, the rebound not reaching in either case within 3° of the acme, and being followed by rapid defervescence.

If experience is worth anything in prognosis, the use of the bath, in these two cases, plainly saved life. Useful as the salicylate of soda may be in acute rheumatism generally, it must be admitted that the foregoing instances demonstrate its inadequacy in certain cases of high fever, and shew that we have in

the bath a remedy capable of completely controlling the hyperpyrexia under these circumstances. The way in which the temperature ran up during the use of the salicylate raises, in fact, a doubt as to whether the salicylate may not have aggravated matters. It will hardly be contended by anyone that the drug had not its full chance, the toxic effects having been well developed. Certainly there is no reason to think that it should have been pushed further, as has been done in some instances on record. My own opinion is that this would have resulted in further poisoning with very little chance of victory over the disease; and that the patients would have died of salicylic acid instead of acute rheumatism.

There follows another consideration of great interest. May the singularly decisive action of the bath have been favoured by the previous full use of salicylic acid? Is the power of checking hyperpyrexia dependent on the same or on different conditions in the two treatments?

In acute rheumatism the existence of two elements at least must be admitted, one local, the other constitutional. As regards the constitutional element several assertions may safely be made.

1. Certain persons are more liable to have acute rheumatism than others.

2. Certain family groups of persons are more liable than other family groups.

3. The persons and families who are thus more liable than the average to acute rheumatism tend to develop hyperpyrexia more than the average. They also tend more to contract, and with severity, other fevers, in particular scarlatina, diphtheria, and enteric fever.

A considerable experience and analysis of family histories justifies me in making the above assertions. They mean that in certain persons the whole system is readily set in flame by provocatives which would be unnoticed by the system of others, that there is greater sensitiveness, greater chemical activity, greater readiness to disintegration. The whole subject of such family and individual febrile diathesis might be followed out. But it would take us at present too far from our proper subject.

The beginning of attacks of acute rheumatism, however, is frequently a local phenomenon. One joint, or perhaps two symmetrical joints, are often inflamed several hours in advance of the general febrility. Such joints are often those which, from the nature of the patient's employment, have more heavy work regularly thrown upon them than other joints, or have been exposed to recent violent strain. In the absence of the particular diathetic state, some aching, some pain, some

stiffness, would be all that would be produced. But in persons prone by stock-inheritance to the acute rheumatic process, or brought into ill health and weak resisting state by unwholesome conditions of life, or more acutely inflammable through the effects of some chill, shock, or systemic strain, the joint sharing in the general weakness—in the general tendency to degeneration—takes on the rheumatic inflammation instead of the simple one, and the system is set on fire from this focus. In the fever which follows, the dangers, and consequently the treatment, must vary according as the series of local inflammations is wide or of grave character, and according as the general febrile action is intense. For, though there is generally high fever, with much joint affection, this rule is not always obeyed, and very frequently the general fever runs to a height altogether above the proportion which it should bear to the local inflammation.

Like blisters placed near joints, or between joints and the trunk, salicylic acid, in all probability, exercises a distinct influence in the repression of the local actions of the disease. It appears to have power to check the inflammatory process, or the disintegrations upon which inflammation depends at the point or points from which the general conflagration starts. The rapid disappearance of pain, and the mitigation of the inflammation of joints, which often follows the administration of one or two doses of salicylic acid, or its salts, afford grounds for such an opinion. These effects are often illustrated in the relapses frequently seen in the course of rheumatic fever, when the joint inflammation comes under notice before the systemic fever is developed, and is easily subdued by a few doses of the remedy before the general temperature is seriously raised. In our present knowledge of the effects of remedies it would not be fair to attribute to salicylic acid merely local influence. The remedy has, to all appearance, considerable power of reducing the temperature of the whole body; but how much by checking local inflammation, and how much by a general influence, I do not pretend to estimate.

The graduated bath, again, has probably a double remedial action, general and local. The nature of the remedy would at first sight suggest that it would diminish the bodily heat in a direct fashion, by simple ablation of excess. The way in which the bath acts, however, indicates that it has a wider function than this. It exercises, in the first instance, a remarkable sedative influence on the nervous system, and in the second place it diminishes the local pain and inflammation. If the chief effect of the bath were to take away so much heat, the use of lower temperatures than those quoted would be required. But if it is evident that we can effect our total purpose of

lowering temperature and arresting morbid processes without producing shock, we may be satisfied with the application of the remedy in the milder form. The shock of plunging a fever patient into a bath at 40°, or less, has been shown by experience to be considerable, and even dangerous. If, therefore, we can satisfy ourselves that the use of the graduated bath involves several modes of action, besides the simple question of specific heat of the body, we shall be putting our practice on a safer as well as a wider basis.

The Bath in Enteric Fever.—The relatively small mortality of this epidemic has been already noted. I venture to claim the reduction as a result of the use of baths. Of the cases in which baths were used, several were clearly arrested in a fatal course. That the bath was not always successful was to be expected. The lesions of enteric fever are so often sufficient of themselves to kill that a remedy which deals chiefly with hyperpyrexia cannot be expected to be a panacea. So far, however, as the danger in any case is purely one of hyperpyrexia, the efficiency of the bath is manifest. The cases in which it succeeded were cases of such a kind; the cases in which it failed were fatal by reason of peritonitis, perforation, or great extent of intestinal glandular lesion with exhausting diarrhœa. Even in these cases the bath has appeared to protract the struggle, affording delay during which more favourable local states might be established. And it may be noted that another external application—the ice bag—has, alone or with the bath, been able to control violent peritoneal inflammation associated with symptoms almost certainly demonstrative of perforation.

A reference to the charts will demonstrate that the effect of the bath was less decisive in the cases of enteric fever than in those of acute rheumatism. The lowering of the temperature was not carried so far, being more often from 6° to 8°, although in one instance, a fall of 11° is recorded (John S.). The subsequent rise was also often rapid, involving generally a speedy repetition of the bath. In the case of John S. the temperature rose within seven hours to a degree above that for which the first bath was given, and four baths were required within twenty-eight hours, the last finally subduing the ascent of fever and introducing a slow but unbroken recovery. In other cases the struggle was further prolonged. Henry J. was bathed eight times between the twenty-third and twenty-eighth days of the fever; and in the case of Louisa W., while the fiercer heat of the second week was kept under by as many as seven baths, cold sponging was resorted to no less than thirteen times afterwards, the last time on the twentieth day, before the force of the fever could be regarded as broken.

The experience of the use of salicylate of soda in enteric fever

agrees with that of the bath in indicating that the more serious and persistent nature of the local lesion reduces the curative effect of the remedy. But both remedies appear to mitigate the intensity of the local process, to weaken its reaction on the system, and to diminish the response of the system to the invitations to excitement proceeding from the part. Here, however, as in acute rheumatism, the bath is the more energetic remedy of the two, and the safer. No evil results of any kind can be traced to its use, the only objections to it resting on grounds of trouble and exposure, such as ordinary care and nursing power ought to exclude. The cases presented in abstract appear to me so satisfactory as to justify, or perhaps even to demand, the use of the bath in all cases of hyperpyrexia in enteric fever where the moving of the patient is not contra-indicated by peritoneal complication or perforation. Even then the cool pack or cold sponging are valuable substitutes. The rule at present adopted with reference to the bath is that it should be used, with the above necessary exceptions, whenever in enteric fever the temperature having reached 105° is still rising, particularly if the rise be before or after the usual evening hour. As I have claimed before, the practice, though not uniformly successful, has not in any instance done any known harm, and has, in all probability, averted death in several instances. Accordingly, notwithstanding some difficulties arising in the carrying out of the process of bathing, I propose to adhere to this rule in all cases of enteric fever coming under my care.—*St. Thomas's Hospital Reports*, vol. viii., 1878, *p.* 129.

5.—TREATMENT OF THE PYREXIA OF ENTERIC FEVER.

By Dr. J. GREIG SMITH, M.A., Medical Superintendent Bristol Royal Infirmary.

Our experience of the methods available for the reduction of temperature in enteric fever has led to a preference for the combined action of quinine and cold bathing. The one is made to supplement the other, and it is thought that the best results are got from the conjoint use of both.

[The other methods recommended are—sponging the surface with cold water, or cold water and vinegar; this is easy of application and grateful to the patient. Roberts's coil, consisting of india-rubber tubing, placed under the patient's back, and a current of cold water passed through it, is troublesome, and not free from risk.]

The *wet sheet* is a powerful antipyretic agent. When only a single thickness is laid on, and the body left without any further covering, evaporation adds considerably to the cooling effect.

Covering with several thicknesses of wetted sheets, evaporation being thus prevented, is simply a clumsy and imperfect method of cold bathing. In private practice, or where no bath is to be had, it might be employed as one of the best substitutes.

One of the most powerful antipyretic agents we have, and one likely, I believe, to be of great value in cases of sudden and intense pyrexia, such as sunstroke, is the encircling of the body in a wet sheet, and rubbing it over with blocks of ice. The direct cold of the ice, added to that got from the ice melting and evaporating on the sheet, give about as much cold as it is safe to apply to the surface of the body.

The most general objections to the wet sheet are that it is not a cleanly process, and that it is equally difficult to test the amount of cold applied and to regulate the effect of its application.

The use of *ice* in connection with the wet sheet I have spoken of. It may also be used as dry cold in flannel or gutta-percha bags. As a physical means of abstracting heat, it is open to the objections that the cold is unequally distributed, that some parts of the skin may be nearly frozen while the rest of the surface is uninfluenced, and that we lose the benefit of cleansing the skin; while its effect through the vaso-motor system has been held forth rather as a suggestion than a demonstration.

The injection of iced water into the large bowel has been recommended in pyrexia; but for evident reasons it is inapplicable in the pyrexia of enteric fever.

In hospital practice, and for most cases, the *cold bath* is probably the best physical means at our disposal for the reduction of the pyrexia of enteric fever. When the system of cold bathing has once got into full swing, it gives less trouble than might be supposed. With us the method is practically that described in the Lancet of June 2, 1877, as in use at the London Fever Hospital. A bath is wheeled to the bedside of the patient, and, by means of pieces of tubing fitted on to the cocks, which supply hot and cold water in the scullery, opening off each ward, it is filled with water of any temperature required. The bath is left filled by the bedside of the patient, and emptied every other day. The patient is lifted in, if a child, by the arms, if an adult by means of the sheet on which he lies, and the head rests on a solid support in the bath. The sheet on which the patient is lifted in is removed to permit of the free circulation of the fluid. A dry sheet laid over the bath will prevent exposure and serve to cover and dry the patient as he is lifted out. For heavy patients a stretcher may be made of an ordinary sheet permitted to bag so as to reach the bottom of the bath, and drawn in at the top so as to form a support for the head; when the process may be gone through without removing

the poles or disturbing the patient. The gradual fall of temperature is shown by a thermometer without an index placed between the cheek and the upper jaw. In the axilla we get only surface temperature, the rectum is not a choice spot; and in both axilla and rectum the temperature indicated is not the exact temperature of the body, but a rough average between that and the temperature of the water.

The effect of the bath on pyrexia will be spoken of further on. With regard to its influence on the system generally I could desire no better description than the following by Dr. McCombie, in the Practitioner for November, 1876:—"In all cases it diminishes restlessness and delirium; in the great majority it produces sleep, and that, too, after the patient has passed many delirious and sleepless days and nights, and even should it fail to produce sleep, almost without exception after its administration the patient doses quietly for some hours. Sleep, as a rule, follows within an hour, and continues, in some cases, two or three hours; in some three or four, and in others even five hours or longer." This is exactly what we have found. The substitution of coolness and gentle perspiration for the heat and clamminess of the skin, and the pleasant languor of slight muscular fatigue, combine to produce a feeling of subjective ease and calm, which has its culminating point in a refreshing sleep. And if we do not have this crowning benefit of sleep we shall probably see, in the diminution of subsultus, the absence of carphologia and the improvement of delirium, decided evidence of the amelioration of nervous disturbance. Apart altogether from the temperature reduction, which is supposed to tell only in the end, we should have in the immediate improvement of the subjective conditions of the patient, almost sufficient justification for the use of the bath.

The effect of the bath on the heart is what we have to dread. If the pulse be no worse, it is all we can reasonably look for. It is nearly always slowed, often much weakened, and not unfrequently, we are told, demands the administration of stimulants. Stimulants we have never once had to give to a patient in the bath, and only once after the patient was removed. This may be owing to the following; first, that the bath is not cooled down after the patient has been immersed; and, secondly, that quinine is employed if the heart's action be feeble. I am confident (though the contrary is very generally believed) that the endeavour to prepare the system for a temperature of 60° by lowering it from one of 90° in half an hour or so causes more depression than to plunge the patient at once into the requisite cold. It certainly causes more discomfort. We all know how much more bathers feel a cold of 60° in the water

when they gradually walk into it from an atmospheric temperature of 80° than when they plunge into it at once. And, though our daily bath at 60° may be a pleasant enough performance, it will be found almost unbearable to have a bath at 90° gradually cooled down even to 70°. The testimony of those of our patients who have had a trial of both plans has been uniformly against the cooling down of the bath. If, then, in the elements of danger to the patient and effect on the temperature there is nothing to choose between one continuous cold of 70° and a cold varying between the extremes of 80° and 60°, we ought to abide by the more agreeable plan. In the belief that gradual cooling of the bath causes more depression, is more disagreeable and not more effectual than keeping the patient in a cold of uniform temperature, we now follow the latter method.

Of the *drugs* in use for the reduction of temperature the best known are salicylic acid and its compounds, quinine and digitalis.

Quinine is the drug which appears to give the best results. It is free of the most serious objection to the salicylates, and in the matter, pure and simple, of reduction of temperature, is at least of equal value. The only postulate is that it shall be given in sufficient quantity. Small doses—and the dose is still small at ten, twenty, or even twenty-five grains in the case of an adult—only tamper with the temperature, without controlling it. For an adult man, 35—40 grains, for an adult woman, 30—35 grains, given in three or four doses in the course of three-quarters of an hour, will probably have the desired effect. Some degree of cinchonism may ensue. There will usually be some deafness. But, in the sense of giving subjective rest and quiet, deafness may be considered rather as a good than an evil. Very rarely there is sickness and vomiting. Nearly always the pulse is improved, often markedly so. We may confidently look for an improvement in the regularity, volume, and rapidity of the pulse beat, and not unfrequently there is decided increase of vascular tension and diminution of dicrotism. Free perspiration is common after the administration of quinine; and once there was marked increase in the urinary secretion. No further effects of any importance have been noted.

I would reiterate the importance of giving the drug in sufficient doses. If we are to expect the full influence of the drug in temperature reduction we must unhesitatingly administer it up to the very furthest limits of safety. In this Infirmary fifty grains have been given by the mouth to an adult male, and the same amount by the rectum to a girl of eighteen with no evil results. Increasing knowledge of the drug gives increasing confidence in its freedom from danger.

The pyrexia of enteric may be better treated by quinine and the bath, than by the bath alone. Where everything is favourable, and where the demands for treatment are not more frequent than twice or thrice a day, we may rest content with cold bathing. With the aid of quinine we may dispense with the heroism of four hundred baths in the course of one case of enteric, and we need not let a pyrexia remain untreated when failing heart's action or complications such as pleurisy, peritonitis, hemorrhage, forbid the bath.

If we have formed an honest estimate of the therapeutic value of both methods, and intelligently applied our knowledge in practice, making the one to supply the defects of the other, and both to work their best towards the common end, it will be seldom indeed that we meet with a case of enteric fever whose pyrexia cannot be kept under control.—*Practitioner, July*, 1878, *p.* 20.

6.—IS INTERMITTENT A MARSH FEVER?

By Dr. JOHN SULLIVAN.

When the human body is exposed to the miasmatic emanations from a marsh, or to those conditions of soil which render marshes unhealthy, the nerve-centres become profoundly impressed, giving rise to phenomena—constant, though varied—marked by periodicity and intermittence.

What is the nature of that unhealthy air or malaria, which exists in and about a marsh? A marsh is a waste, uncultivated piece of land, very damp, or partially covered with water, which does not dry up, but remains stagnant. In this description may be included all malarial localities. Although water may not be visible on the surface, nevertheless water will be found beneath the surface, imprisoned, circumscribed, and mixed with organic *débris*, where, under the influence of heat, it will ferment, putrefy, and exhale through the porous earth a miasma identical in its effects and in its nature with that exhaled from an unhealthy marsh above ground.

When water from any source falls upon any soil, a portion of this water will pass off as vapour; the remainder will filter through, and, if it meets with no obstacle, will be absorbed and totally disappear. Whatever tends to oppose the progress of this filtering water promotes the formation of a marsh. It is for this reason that, when the subsoil is argillaceous, or of a tertiary formation, or a tufa of volcanic origin, as may be noticed in the Campagna of Rome, in Tuscany, along the coast of the Mediterranean, it then forms the basis of some very malarial soils. If the soil be thickly wooded, and clothed in rank vegetation, evaporation beneath the shade of lofty umbra-

geous trees, as in the virgin forests of India or America, is checked, the hot oppressive air is confined, ventilation is obstructed, the water is detained by heaps of decayed vegetable matter, or will filter through, and thus augment the streams and rivers, which, fed from this source, seldom dry up in the tropics, even in the driest season of the year. The malaria in such localities is very pernicious. The springs in some places are exhausted during the dry season from the absence of water to filter through. The rivers no longer supplied, unable to convey to the sea the water which stagnates in their beds, become the depositories here and there of unhealthy marshes. Whenever we meet with a bar or sandbank at the mouths or estuaries of rivers, where the land lies low and flat, it will be the result of the incessant conflict between the ascending and flowing sea-wave and the descending waters of the river. The bar presents an obstacle to these waters, which, after mixing with sea-water, overflow their banks, and inundate the adjoining lands; and thus deltas, islets, and swamps are formed, the hotbeds of marsh infection in hot climates.

A marsh favours the development of a peculiar class of vegetation. It is the receptacle of a constantly renewed organic creation, and of the process of an incessant putrefaction. A marsh, to quote an ingenious Italian writer, "is the mysterious laboratory of life and death. It is the cradle and grave of an innumerable variety of plants and animalcules. A marsh presents a striking contrast between the stagnant immobility of its silent water, and the mobility and agitation of the myriads of animated beings which it shelters. And hence, to protect unmolested the orgies and gambols of an unclean creation, a marsh opposes, in order to ward off the approach of man, a barrier of infection and disease." It often presents a shining bluish appearance. The colour and transparency of its water may be modified by the nature of the elements held in suspension, some in a partial, others in a complete state of decomposition; or there may be present infinite quantities of microscopic parasites, some imparting a green, others a reddish colour to the stagnant water, the odour from which is sometimes fetid and very repulsive.

The water of a marsh may, by its density, its temperature, and the degree of its saturation, interfere with the process of fermentation in a fermentable body; but if another liquid be added to it, even the water of another marsh, but more especially if salt and fresh water become mixed, by which mixture sulphuretted hydrogen gas is generated, fatal to organic life, the conditions of a marsh become greatly altered. Any change in the medium of the water of a marsh will prove fatal to the life of the innumerable plants and animalcules which live in it;

they perish and are converted into putrefying matter. If a marsh abound in water, insect and vegetable life is preserved—no putrefaction takes place; but when the quantity of water is diminished as it dries up, exposed to the rays of a burning sun, the malaria from a marsh is specially noxious. The water of an unhealthy marsh contains various deleterious gases. In many of the Tuscan marshes, if the water be stirred up with a cane or rod, the gases that arise are inflammable. Marsh malaria contains organised microscopic creations—microzymes, micrococci, spores, &c.—the product of incessant fermentation and putrefaction; and this malaria may be conveyed to the mucous membranes of the human body by contact, by inhalation, or by the agency of the wind.

Now, this fermentation in a marsh leads us naturally to a consideration of the germ-theory of biogenesis: life springs from antecedent life.

If an ear of tainted or corrupt corn be suspended in a bottle of muddy water, we shall soon observe that as some microzymes die which people this marsh in miniature, infusoria and bacteria become developed. We are thus led to infer that marsh infection is the product of the absorption of animated sporules; or, as Lancisi terms them, "animated effluvia." Life is the consequence of antecedent life; spontaneous generation is impossible.

Myriads of organised creations abound in marshes, which, by means of a locomotive apparatus consisting of agile filaments or ciliary points, move about instinctively and with great rapidity, some to find a soil suitable for germination, others for some fixed point of rest, vegetable or animal juices, to which they might attach themselves as parasites and multiply. It is interesting to inquire which amongst the varied creations which people a marsh is the most capable of generating that morbid agent in malaria which gives origin to fever and ague.

Lancisi, Torrigi, Ballestra, and other Italian writers, are of opinion that marsh malaria is composed of the sporules contained in the protoplasm of the cells of an alga which abounds in marshes, and which they have frequently detected in the effluvia from the Pontine Marshes.

What is the nature of that morbid agent to which the miasma or malaria from marshes owes its special action? No other morbid cause produces the same morbid effect. This agent, although invisible, must therefore be special and material. The term "miasma" implies corruption, the product of that fermentation and putrefaction of organic matter which constantly exists in marshes. It is a compound of heterogeneous elements from vegeto-animal decomposition, diffused in the air above and around a marsh; and whereas this infectious exhalation, when

introduced into the human organism, is incapable of undergoing unlimited multiplication, it cannot be communicable or contagious. Its immediate action when absorbed is nerve-stimulation; its natural action is nerve-depression, or paralysis. Nerve-paralysis must be preceded by nerve-stimulus. This produces, in marsh fever, contraction of the arterioles in the periphery, derivation of blood to internal organs, increased internal temperature, sensation of cold and shivering; while the consequent nerve-paralysis induces heat in the periphery, increased vascular action, and, finally, general relaxation and sweating. This poison manifests itself in a series of paroxysms, or explosions of fever, followed by intermittence.

Nerve-energy may resist to a certain point all accumulating disturbing agencies, when it will suddenly give way to a fresh manifestation of the phenomena of fever and ague. Such phenomena may be observed in the sudden explosions of periodic neuralgia, epilepsy, asthma, &c., under conditions of body in which some cause of irritation productive of these diseases constantly exists. An anæmic state of the blood is characteristic of these diseases, which, as well as fever and ague, are of nervous origin. But periodicity is more marked in diseases of a malarial origin than in any other disease due to a disturbance of the nerve-centres. Graves, in his inquiries into the law which regulates a relapse of fever and ague, arrives at the conclusion that the law of periodicity which governs the disease, while it occasions fits, continues likewise to preside over its latent movements when no fits occur; so that a relapse of fever and ague, after a suspension of many months, may appear on the very day on which the fit would have occurred had no suspension taken place.

Since, therefore, exposure to the conditions which render a marsh unhealthy, generates in the human organism a specific febrile condition marked by periodicity and intermittence, intermittent may with great propriety be termed a marsh fever.

Is marsh malaria a gaseous body, which specially so paralyses the nerve-centres as to give origin to a series of characteristic phenomena, as carbonic acid gas causes special nerve irritation and spasm of the glottis? Or does the action of malaria depend on the presence of special living germs evolved during fermentation in a marsh?

Without the contact of air, which involves the germs of impregnation, neither fermentation nor putrefaction can take place; and it has been proved by Professor Lister that even lactic fermentation cannot possibly take place in the absence of the impregnating influence of atmospheric air. The air we breathe must therefore involve living germs. Even the blue ether above us, as seen from the highest elevation, far away

from any possible contamination with the earth, teems with infinite invisible floating bodies, not to be detected by a microscope of the greatest known power, but made visible when a stream of electric light is thrown upon them, as practised by Professor Tyndall. This eminent man has exhibited through the same medium the contagium in a sample of old hay, involved in clouds of fine particles, which rose up when the hay was shaken; and this contagium was more infectious than that usually found in common air, capable of influencing the process of brewing and other like products. What germs may we not expect to be derived from the putrefaction of animal and vegetable origin in a foul marsh!

Dr. Burdon-Sanderson has been able to obtain from an aqueous infusion of putrid animal tissue an organic substance—"sepsin"—to which septic fluids owe their virulence. He has proved that the serum of blood, if kept excluded from contact with air, will remain intact; whereas, if it be impregnated with the most minute particle of this septic extract, it will determine the development of bacteria, spirillæ, and a variety of organisms. But the malaria or miasmatic septic effluvium from an unhealthy marsh has hitherto resisted all attempts and systems of analysis; nevertheless, the existence of malaria, the effects of which on the human organism are constant and uniform in their nature, although not appreciable by the senses, should no more be doubted than that typhoid or enteric fever is the product of the effluvia from foul sewage.

Pouchet, of Rouen, is so firm a believer in the notion of a spontaneous generation that he considers the "germ theory" to be inconsistent with the idea of a Creative Wisdom, I, for one, believe that creative wisdom is most effectually displayed in the observance of the germ theory in obedience to the mandate in the Divine Word: "God created every living thing that moveth, saying, Be fruitful and multiply." The Creator gave the first impulse to universal life in every form, however minute and apparently insignificant, which can only be perpetuated by antecedent life.

A prominent member of our profession not only disbelieves in the agency of living propagating germs, but also in their very existence. He advocates a "glandular theory," that diseased glandular secretion is at the root of some communicable diseases which he calls "septinic, not zymotic." If of some, why not of all? A theory which does not apply to all diseases of a class is no theory at all. Poisons, no doubt, direct their energy to one tissue in preference to another, or to one particular gland or series of glands for which they may have an affinity. Hence, we detect disease of Peyer's glands in typhoid, of the mucous glands of the throat in

diphtheria, of the secretions of the nasal surface in glanders, &c. But any glandular secretion that may form in the course of a communicable disease, although it may propagate, does not originate, but is the consequence of the specific poison which produces the disease.

Now, no matter or glandular secretion taken from the subject of fever and ague can by any possibility reproduce the disease. I cannot, therefore, comprehend how "ague" can be reckoned in the number of communicable diseases.

"If it were true," this physician continues, "that every contagious disease is due to its external independent living germ, that these germs continue to increase and multiply, why do these germs after a time cease to multiply, and allow the patient to live?"

The air above and around us teems with invisible life—not opposed to healthy vital action, but capable when tainted by disease from any source, be it local or conveyed from afar, of communicating the disease from an unhealthy to a healthy locality, from an infected to a healthy subject. Man may be surrounded by elements tending towards disintegration and destructive of life; but life in health, sustained by nerve-energy, is capable of resisting these elements. When this nerve-energy becomes enfeebled as the health declines, when the tendency to decay manifests itself, this cohesive resistance will yield to the influence of the surrounding germs, ever prone to attach themselves to unresisting organic unhealthy matter, to consume the nitrogen of the tissues, and thus complete their disintegration and decay.

Two patients are seized with the same contagious disease, the one in a mild, the other in a severe form. Both are exposed to the same diseased germs which have served as the vehicle of the poison. Both would be equally affected by these germs did not the healthy organic resistance in the one prove superior to that of the other. While a third person may be exposed to the same agency, and not become affected at all.

If all men were susceptible of equal impressions from diseased germs or atmospheric influences, then the superior must yield to an inferior organisation, and the entire race of man on the earth's surface must perish. The germs of disease cease to multiply because their action is neutralised by the superior influences of improved healthy vital action in convalescence tending towards cohesion, opposed to feeble action tending to dissolution and decay. Healthy tissues are an unsuitable soil for such germs: they cannot attach themselves as parasites; they can neither grow nor multiply in them.

"No man," continues the authority from whom I quote, "has ever seen a diseased germ. We can study the yeast growth, its

causes, its action, the changes it produces ; in epidemic diseases we have no such guide."

Nobody doubts the existence of a noxious gaseous body, although it be invisible.

Now, yeast, which is the development during alcoholic fermentation of a microscopic cellular organism—the yeast plant, "torula cerevisiæ," capable of multiplication, is the cause of this fermentation, and when introduced into a fermentable body acts as a ferment. So will the contagium in a communicable disease act as a ferment in a healthy body, produce its own special phenomena (the coexistence of which in the body, as Dr. Maclagan says, is conveniently described as fever), and reproduce the communicable disease. And as we can watch and observe the action and progress of the yeast plant, so can we study the origin, action, and progress of the special contagium.

"The matter," he again says, "which sets up the poisonous action in a living body is actually dead matter."

Now, what he calls dead matter is the organised product of the disease during its development and progress, which disease was caused by an external agent, but which product is often capable of reproducing the disease in the course of which it was formed. Vaccine matter owes its activity to transparent microscopic organised granules capable of transmitting the cow-pox; but the cow from which the matter had originally been taken had never been vaccinated with such matter.

Why do atmospheric agencies, in that subtle region where electricity, magnetism, motion, and sound execute their invisible interchanges, exert so great an influence on the organism in the causation of disease? Sudden variations of temperature at certain seasons of the year, causing violent electric disturbance, or sudden displacements of air, to be replaced by a violent rush of air from a far distance accompanied by rains or by storms, may influence the human organism; but these atmospheric phenomena are transient in their effects. Any morbid effect produced by atmospheric influence must be attributed to some noxious principle involved in the air, modified by soil or by climate—to the presence of invisible living germs, which are attracted by unhealthy animal or vegetable tissue, and attach themselves (if not prevented by antiseptic means) to dead tissue, causing putrefaction and resolution. A host of modified germs, tainted with a specific contagium, may suddenly invade a locality, and generate a specific epidemic. Do we not often find a flourishing field of corn suffer from a blight, and droop in the course of a single night?

When the writer asserts that during a prevailing epidemic many cases are traceable to nervous impression, distinct from

cases due to direct contagion, it appears to me that he adduces a strong argument against his favourite theory. The impression of fear cannot, independent of any other cause, produce an attack, say of cholera. Fear, by debilitating or paralysing nerve-energy, may render the system more susceptible of a special impression from the cause of the disease. Peculiar atmospheric conditions, involving specific germs, acting upon an organism rendered more susceptible from fear, will impress the vasculo-motor system of nerves, affect the circulation and composition of the blood, accompanied by the phenomena peculiar to the epidemic.

When we reflect on the influences created by the nature and quality of the air we breathe, or the many proofs we daily witness of the efficacy of the antiseptic treatment in disease, attested by the experiments and writings of the most eminent teachers and philosophers of our day, both at home and abroad, we are compelled to incline to the belief that the germ theory of disease is the more consonant with the laws of nature, of creative wisdom, and the best calculated to promote the social and sanitary condition of a people.—*Medical Times and Gazette, Oct.* 12, 1878, *p.* 432.

7.—THE ACTION OF MALARIA AND ITS INFLUENCE ON THE SPLEEN.

By Dr. JOHN SULLIVAN.

Malaria, an element derived from marsh infection, whether it exist under a gaseous form, or as an emanation evolving special diseased germs, which paralyses that system of nerves that regulates and controls the arteries and the circulation of the blood, which creates a condition of hyperæmia and congestion opposed to all plastic and true inflammation, becomes a source of great danger when complicated with some local affection of any organ essential to life, as of the liver, the lungs, kidneys, &c. And this malarial fever (a compound of a local and general disease, which mutually react and aggravate each other) is called pernicious, or dangerous to life.

The action of malaria varies with the individual exposed to its influence; and this difference of action will depend on the degree of its intensity, the constitution of the individual, and upon that condition of body created by past disease, or the predisposition to future disease. When malaria exists in a latent or masked form in the human body, it will excite and complicate any disease to which the body, from some peculiar temperament—be it nervous or sanguineous—may be disposed. Thus, if the constitution be disposed to asthma or to hysteria, the patient under the influence of malaria may be seized with a

pernicious asthmatic or a pernicious hysterical fever. Should he be disposed to rheumatism or to liver disease, there may be a complication of pernicious icteric or pernicious endocarditis. The form of malarial disease will always be characterised by some prominent symptom of the special diathesis.

Professor Baccelli, of the University of Rome, mentions the case of a young midwife in whom malaria existed in a masked state; who became so deeply affected by the dreadful sufferings of a patient in her first confinement, together with the sight of the inhuman conduct of a brutal husband on the occasion, that on returning to her home she was seized with a violent "lumbo-abdominal neuralgic pernicious fever"—that is to say, that the malarial poison excited and directed certain pains to parts already impressed from previous associations; pains exactly resembling those from which her patient had so severely suffered.

This peculiar paroxysm of malarial fever repeated itself three times, and finally yielded to the administration of quinine in large doses. The germs of malaria, when absorbed in the human body, act as ferments. The poison accumulates, and at certain regular intervals of time of intermittence it will explode, discharge, and finally exhaust itself, until a fresh charge is generated by causes from without or by causes within the body, attended by phenomena transitory as are the paroxysms of marsh fever, not constant as may be observed in the course of those remittent or continued fevers which do not owe their origin to the poison of malaria. These special disease-germs, when absorbed, act on the ganglionic nervous system and on the blood corpuscles, causing vaso-motor irritation, followed by vaso-motor paralysis.

Of poisons which act on the nerves, and through these on the muscular fibres, some will cause continued nerve-irritation and muscular spasm; and if this effect be too long continued, and not followed by relaxation or nerve-paralysis, death must result from spasm and suffocation. Other poisons may cause immediate nerve-paralysis, and death may result from exhaustion; but marsh malarial poison always produces nerve-irritation, followed by nerve-paralysis.

In a paroxysm of malarial fever there is an abnormal production of heat, from the effect of the poison on the nerve-centres and the heat-producing powers; there is nerve-paralysis, and consequent increase of temperature and congestion. This increase of temperature in marsh fever is supposed by many Italian writers to be more the effect of nerve-paralysis than of any other cause of increased combustion and tissue-metamorphosis, since this abnormal heat is not followed in the early stages of malarial fever by any profound modification in the process of nutrition.

Neither do they consider that it depends on the amount of urea excreted, although in marsh cachexia and latent malaria this excretion is often a more delicate test of the degree of tissue-metamorphosis, and of the consequent amount of heat produced, than is the thermometer.

The deposit of urates during a paroxysm of fever and ague varies greatly. It may be altogether absent, or not at all in proportion to the violence of the fever.

We detect by means of the thermometer, even before and during the cold stage, great elevation of temperature; while that of the periphery is lowered from a contraction of the smaller arteries through primary nerve-irritation and a diminished tissue-metamorphosis. The body-temperature cools down during the hot and sweating stages, and by the relaxation of the peripheral vessels heat is given off.

The action of the poison of malaria upon a particular system of nerves produces an effect similar to that which would result from their division—such phenomena as actually occur when a branch of the great sympathetic going to any particular part of the body is divided and consequently paralysed,—and the parts supplied by it would become hot, swollen, and congested, the subject of hyperæmia and of serous exudations, not of inflammation, in the absence of the controlling power of the vaso-motor nerves over the arteries.

It is related in the Archives of Virchow that in a case where the nerves (the splenic plexus of the great sympathetic) which go to the spleen had been divided, there ensued excessive hyperæmia and degeneration of tissue. These changes may therefore, be regarded as the expression of a morbid nervous influence. Marsh fever is the declared reaction of the living organism against the cause of infection; it is based on the principle of anæmic congestion, not of true inflammation. As in pneumonia there exists a stasis or paralysed condition of the red corpuscles of the blood, as they cannot convey oxygen to the tissues, so the action of marsh malaria, although more slow, is not less injurious to the red corpuscles. They shrink, from the want of their natural supply of oxygen, and a serious injury is inflicted on the respiratory portion of the medulla spinalis through the action of this altered blood when the function of the red corpuscles is arrested. We may find an explanation for the dyspnœa in pneumonia in the exudation poured into the parenchyma of the lungs, or in the attendant fever; but in marsh cachexia there may be no fever, and although there may exist no cause capable of encroaching upon the area of the lungs, dyspnœa will be observed, from a deficiency of the red corpuscles, the carriers which supply the oxygen necessary for combustion.

The spleen is the organ which suffers most frequently from the influence of the poison of malaria; a reservoir of nutritive blood materials, to be drawn upon or utilised when required. An organ void of ducts, not intended for secretion, but for the elaboration and assimilation of germ-cells, the materials of which are derived from the blood, must necessarily suffer in an especial manner from the action of a poison like malaria, which directly tends to decompose this blood, from which it derives the materials for the performance of its function.

Under the influence of malaria the spleen will pass from a slight transient hyperæmia to a permanent condition of hypertrophy, to alteration of texture, degeneration, and atrophy. If the poisonous action be not continued, the swelling may disappear, whether through the spontaneous interruption of the febrile process, or under the administration of quinine. If the action be continued, as in marsh cachexia, the swelling of the spleen will be constant, its texture will abound in white corpuscles shrunk and altered, or the gland may be atrophied. The sensation of cold alternating with heats, the livid hue of the skin, and the exudations of serum, afford evidence of cerebro-spinal and pulmonary anæmia. The black pigment so often detected after death from marsh cachexia and pernicious fevers, in the brain, lungs, liver, blood, &c., the product of the ultimate disintegration of the plasma and corpuscles of the blood, is supposed to have its origin in the spleen, whence it is conveyed through the abdominal veins.—*Medical Times and Gazette*, *May* 11, 1878, *p.* 504.

8.—MALARIA AND AGUE.

By Henry Frederick Norbury, Esq., Staff Surgeon, R.N.

A considerable amount of scepticism seems to be rife at present as to the actual existence of malaria as a specific poison, and, in conversations I have had with medical men of experience in the tropics, I have been surprised to find how many adherents this heresy possesses; indeed, a work was published no long time since which claims to have proved the non-existence of malaria as a producer of intermittent fever, and attributing the phenomena to the effect of "chills" or the rapid abstraction of animal heat. A considerable experience in different parts of the world has served to strengthen my belief in the popular doctrine, and the following case which some time ago happened to myself is so very clear in its etiology, and the cause and effect are so palpably connected, that no evidence could in such an instance be stronger, and under these circumstances it is, perhaps, not undeserving of record.

The Cape Colony is somewhat celebrated for its freedom from

agues, at least such is the case with its south-western portion; indeed, on the peninsula terminating in the Cape of Good Hope, and on which Simon's Town, the principal naval station, is situated, such diseases are absolutely unknown. This peninsula consists of a chain of rugged mountains acquiring a height of 2000 feet, gradually sloping on the east towards the sea, while on the west a small sandy plain intervenes. The geological formation comprises strata of non-fossiliferous red sandstone, several hundred feet in thickness, based on granite and gneiss, while mounds of gravel are occasionally met with; there is no collection of subsoil water, and the surface is covered with sandy humus thickly studded with bushes; the potable water, moreover, derived from the rain which has percolated the sandstone, is singularly free from organic matter, as I have ascertained by frequent testing.

The present season has been remarkable for its heavy rains, a few very bright hot days intervening. It was on one of these latter occasions, while taking a walk with a friend, that I stood on a heap of moist decaying vegetable matter by the roadside, principally composed of the thick succulent leaves of the Opuntia vulgaris, so as to get a better view of the scenery. The sun was shining brightly on it at the time, when suddenly I inhaled an odour so rank with the peculiar exhalation of decomposing vegetation that I felt nauseated, and was obliged to abandon my position. Exactly four days subsequently, towards noon, I began to feel languid and experienced severe aching pains in the back and limbs, and in the course of an hour I had a violent fit of shivering, with chattering of the teeth, cutis anserina, lividity of the toes and fingers—indeed, all the usual symptoms pertaining to the cold stage of ague. This lasted for an hour and a quarter, during which time I was given hot drinks. Hot-water tins were applied to the surface, &c.; and then the stage of pyrexia came on, the temperature soon rising to 101·4°, and eventually attaining that of 105·2°. This lasted for an hour and three-quarters, and then began to be succeeded by the sweating stage; the diaphoresis was profuse, completely saturating the mattress, bed-clothes, &c., and leaving me quite debilitated. Immediately that these symptoms had subsided I took a purgative, combined with a ten-grain dose of quinine, continuing five-grain doses at intervals until some amount of chinchonism was felt. Two days subsequently I underwent a very mild paroxysm, since which I have had no return. I should remark that I had not quitted the district for three months, nor had I been out of the colony for nearly a year, neither have I ever before experienced the slightest symptom of ague.

I afterwards examined the matters contained in the air about

the heap by the process of condensing its moisture on a large test-tube filled with ice, and submitting the fluid to the microscope; but a power of 1300 diameters showed nothing unusual, although the same fluid caused speedy decolorisation in a solution of permanganate of potash, and gave a faint violet tint on ebullition with chloride of gold.—*Lancet, Oct.* 5, 1878, *p.* 476.

9.—CONCERNING THE INFLUENCE OF PERSPIRATION ON THE FEBRILE TEMPERATURE.

By Dr. SYDNEY RINGER, Professor of Medicine at University College, London.

This preternatural heat of the body has been accounted for in different ways. Some ascribe it to the dry skin in fever, whereby less heat is lost by evaporation; whilst the production of heat is maintained, the excess accumulates in the body and raises its temperature to fever height. If the production of heat remains the same whilst less is lost by evaporation, heat of course must to some extent accumulate in the body, and so raise its temperature; but this accumulation of heat, as I shall shortly show, plays a very insignificant part in the generation of fever.

According to another, and, as I believe, the correct view, by far the greater part of the preternatural heat of fever is due to increased formation of heat by increased combustion of the tissues, especially the nitrogenous tissues. During fever the quantity of urea is largely increased, and this is generally considered to depend on general combustion of the nitrogenous tissues. Some have demurred to this conclusion, maintaining that the increase of urea is due to the retrograde metamorphosis of morbid products formed by the disease, as the exudation into the lungs of pneumonia. This explanation, however, fails altogether to explain the increase of urea in an attack of ague, where no morbid products are formed. Some years ago I showed that during a paroxysm of ague the urea is increased in proportion to the height and duration of the fever, so that given the height of the fever we can approximately calculate the increase in the urea; and, *vice versâ*, given the increase of urea, we can ascertain the height of the fever. The increase of the urea begins directly the fever begins, and declines with its decline. This increase in the urea must be ascribed to increased combustion of nitrogenous tissue; and as the increase of urea is in proportion to the severity of the fever, it is fair to conclude that this increased combustion causes the fever.

A case of rheumatic fever with high temperature but with freely perspiring skin conclusively proves that fever is not simply due to accumulation of heat through loss of evaporation

from a dry skin. In other febrile diseases, too, as in typhoid fever, especially where there is considerable exhaustion, the skin may be occasionally moist, and even soaked, whilst the temperature is very high.

I will now adduce additional evidence to show how little share the dry skin plays in the production of the febrile temperature. Two patients were admitted under my care with ague. I determined to excite profuse perspiration before, or just before, the commencement of the febrile paroxysm, and to watch what effect this free perspiration might have on the high temperature.

The first patient suffered from quotidian ague, and his temperature rose in an untreated paroxysm to 105° and 106° F. Just before the onset of an attack, I gave him half a grain of pilocarpine, which in twenty minutes produced copious perspiration; yet, in spite of this, the temperature rose six degrees, to 104·4°, and the fit lasted as long as on previous days, the temperature falling short of the attacks on the previous days by about a degree. As in ague, the untreated fits often differ to a greater degree than this, it is doubtful if even this slight diminution was due to the jaborandi. I may mention that the sweating produced by the jaborandi had very little influence on the shivering, and blueness of the lips, nose, and extremities.

The next patient suffered from irregular tertian fever caught in Florida. In an untreated attack, on August 1st, his temperature rose to 104·8°. On August 4th the rigor began at 3.20, his temperature at that time standing at 101° F., conforming to the rule with ague, that the temperature rises one or two degrees before the rigor begins. Five minutes after the beginning of the rigor, Mr. Neale, my resident assistant, administered hypodermically a quarter of a grain of pilocarpine. In a quarter of an hour perspiration began, the temperature standing at this time at 102·6°. The perspiration soon became profuse, and yet at 4.30, fifty minutes after the commencement of free perspiration, the temperature stood at 105·5°, and continued above 105° till 5.30 p.m., when the fever began to decline, and the temperature became normal between 1 and 3 a.m. on the following morning, the fit lasting more than ten hours. On August 7th he had a fit which was untreated. On August 10th he had another attack. At 3.20 his temperature was 101·8°. At 3.30 Mr. Neale gave him a hypodermic injection containing half a grain of pilocarpine. At 3.45 he was perspiring very freely, and his temperature marked 102·1°. At 5.30 the temperature was 105°, and subsequently rose to 105·2°. It remained at or above 105° till 8 p.m., and then fell, becoming normal at 4 the following morning; the fit, therefore, lasted over twelve hours. On August 13th he had another attack.

At 5 p.m. his temperature was 101°. At 5.30 half a grain of pilocarpine was administered hypodermically. At 5.45 he was sweating, and his temperature then marked 103°. At 7.15 his temperature stood at 105·4°, and so remained till 8 p.m., and after this time it fell, becoming normal between 3 and 5 the following morning. In this case the attack lasted more than ten hours.

In these experiments, then, the temperature in an untreated attack rose to 104·8°. In the three fits treated with pilocarpine, which produced copious perspiration, the temperature reached respectively 105·6°, 105·2°, and 105·4°, the fits lasting, respectively, ten, twelve, and ten hours. We may therefore fairly conclude that the free perspiration had a very insignificant influence on the febrile temperature, and the increased heat cannot be explained by its accumulation owing to a dry skin, but must be due to increased production of heat from increased combustion.

In his interesting and suggestive lectures on Cardiac Depressants, Dr. Fothergill explains the effect of aconite and tartar emetic on the febrile temperature by their changing the dry to a moist perspiring skin, and so increasing the loss of body heat by increasing radiation and evaporation. As I have already said, by making a dry skin moist we must of course abstract a certain amount of heat by evaporation, and to this extent cool the patient; but the experiments given in this paper show, I think, how insignificant a part the loss of heat, induced in this way, plays in causing that great fall of temperature so often produced by aconite or tartar emetic. Other reasons may be adduced in support of the same conclusion.

1. Whenever aconite promotes perspiration, a proportionate reduction of temperature ought to take place in all diseases; but whilst, in many cases, as in tonsillitis, &c., the fall of temperature is considerable, in other forms of fever, though the perspiration may be very free, yet scarcely any, or even no fall of the temperature takes place; for instance, in many cases of erysipelas, pneumonia, pleurisy, and especially in the specific fevers, the fever continues unchecked.

2. We, not uncommonly, find that aconite quickly reduces temperature without promoting sweating, especially with children, in whom this drug in many instances fails to produce it.

3. Sometimes we see cases like the following: In typhoid or scarlet fever a patient with a hot dry skin, to whom we give aconite, becomes in a few hours freely bathed with perspiration, which continues several days, and then, in spite of the drug, the skin again becomes quite dry. Now in a case like this we find that the temperature undergoes no change. It remains as

high during the sweating as before giving aconite, and does not rise on the cessation of the perspiration.

4. Some years ago, in conjunction with Mr. P. Gould, in order to test the influence of perspiration on the temperature, we three times performed the following experiment:—We placed a fever patient in a hot-air bath, with the exception of the head and face. When free perspiration came on the bath was removed and the patient covered lightly with clothes, and in this state he sweated for some hours afterwards. Whilst in the hot-air bath his temperature did not rise, nor did it fall after the bath, notwithstanding the free perspiration and light clothing. If it be objected that the clothing prevented evaporation, and the consequent reduction of temperature, I may reply that these are the identical conditions under which aconite in so many instances causes so marked a fall of temperature.—*Lancet, Oct. 5*, 1878, *p.* 473.

10.—GUN COTTON AS A DISINFECTANT.

By Dr. F. E. JENCKEN, Kingstown, Ireland.

It is certain that nothing has created so great a revolution in the practice of surgery as the application of the germ hypothesis to this useful and important art, especially in reference to the treatment and dressing of wounds, or, in other words, to the use of antiseptics in breaches of continuity, from the smallest abrasions of surface to injuries of the most formidable kind, whether produced by cutting instruments or inflicted by missiles of a dangerous and destructive character, as in gunshot or other lacerated wounds.

But though the employment of antiseptics, in the manner just alluded to, is comparatively of modern date, chemists have long been aware that certain substances have the power of retarding, or altogether preventing, fermentation, either by destroying the life of the ferment or by substituting a body which, though indifferent in itself, possesses nevertheless the property of supplanting the ferment in question, thus staying the progress of its action. Such are glycerine, sugar, and oil, their surgical action not so much depending upon any chemical properties they possess, as upon their protective power and the influence they have in modifying and improving the secretions; many volatile hydrocarbons of a vegetable order, as the essential oils of mint, thyme, sage, bergamot, and hops, or the resins and balsams, as tar, carbolic acid, creasote, myrrh, benzoin, the balsams of Peru and Copaiba; again certain chemicals, as corrosive sublimate, hydrocyanic acid, chlorine and its allies, the sulphurets and the sulphites, under all of which animal and vegetable life becomes extinct, succumbing in each case, or

nearly so, to the overruling force of the particular agent employed. But certain as their destructive power over every form of primitive life is, and universal as the introduction of some of these agents into medicine and surgery has been, they nevertheless possess certain disadvantages that continually crop up, and are keenly felt by such as are in the habit of resorting to their use, not the least of which is the irritation caused by their application to raw and exquisitely sensitive surfaces.

Carbolic acid, the antiseptic most in use, has this great drawback in common with its correlatives; hence the great discrepancy amongst professional men as to the proportion in which it should be employed—some recommending it in its pure form, as in Lister's dressing, whilst others dilute it in the proportion of forty to eighty parts of any convenient vehicle, this attenuation being even considered by some as not sufficiently low. Another disadvantage, though not so great as the former, is its disagreeable smell, which, though not exactly nauseous, is to many intolerable.

Salicylic acid has certainly none of this inconvenience, but then it is somewhat more irritating, and less efficacious; whilst others are open to still greater objections. It would therefore be a great desideratum if a material were discovered which, in possessing strong disinfecting properties, should, at the same time, not mar the progress of the cure by unnecessarily causing pain or irritation to the wound. Thymol bids fair to be such an agent; but we know too little about it to speak of it unconditionally, and only trust the profession will neglect no opportunity of investigating this new and promising antiseptic.

But, irrespective of chemical agents, we must, to a very great extent, depend upon mechanical means for the prevention of infection, as cleanliness, ventilation, and pure air, but, above all, upon the protection of wounded and exposed surfaces from the influence of the atmosphere. This has from the earliest times been done in an infinite variety of ways, even to the light gauze bandage of modern times; but, useful as many of these appliances are as mere protectives, and for the purpose of aërial filtration, they must, from their very nature, fall short of accomplishing the important end we have in view, *i.e.*, that of disinfection; and here we would venture to recommend the employment of gun-cotton, both as being a light and cleanly material, as well as, from the mode of its preparation, a covering of a decidedly antiseptic character. Gun-cotton, as is well known, is prepared by immersing cotton-wool in strong nitric and sulphuric acids, and then washing it in many successive waters, until every trace of acid has disappeared. It is thus rendered so extremely loose as to its cohesive force as rapidly to explode upon a smart blow of a hammer or any other blunt instru-

ment; but what is more to our immediate purpose, by having gone through the process of double acidification it has become, so to speak, like dynamic gunpowder. It is, therefore, exceedingly probable that, notwithstanding the repeated washings it has undergone, the principle of the acids in which it has been immersed still clings to its delicate fibres, and that, though extinct as to its chemical action, it still remains in its meshes in sufficient force to act the part of a positive disinfectant, without the drawback of being an irritant—a disadvantage we had to complain of when speaking of the other antiseptics.

Requiring the careful manipulation that it does, great caution should be exercised in the beginning in ascertaining the extent of its detonating properties, even under a moderate heat—say from 96° to 120° Fahr.—whether it would bear the warmth of the body, and to what extent. Again, it would be necessary to discover the amount of friction it would endure with safety, as the rubbing of the bedclothes and bandages, turning of the patient, &c.; and lastly, we should make sure of its not being liable to suffer from prolonged pressure, such as the close covering inseparable from surgical appliance, &c. All these matters would have to be carefully gone into, and the feasibility as to its employment thoroughly ascertained before resorting to its use. For my own part, I should scarcely apprehend any danger as likely to arise from any such tendency, yet, upon general principles, when any new experiment is tried, caution is the first and most imperative rule.

Collodion has long since been in use, but, from its liquid character and the disposition it has to scale off, it can only be applied to superficial sores and ulcerations, not to speak of its etherial menstruum by which the pristine character of the cotton has been considerably altered. Flexible collodion is certainly more manageable, but also a great deal more irritating, being still further removed from the original material owing to its admixture with Canadian balsam and castor oil. Gun-cotton, on the contrary, has none of these objections, and though its molecular arrangement has been subjected to a considerable disturbance, verging as it does on the eve of chemical transformation, it is free from every medication in the ordinary acceptation of the term, retaining its valuable properties, to all outward appearance, as before.

Should the above surmises turn out to be correct, gauze, prepared in the manner of gun-cotton, may be employed instead, either in its simple form, or rendered impervious to the air by steeping it in spermaceti and resin, as in Lister's dressing. I regret I am prevented from testing the efficacy of gun-cotton as a surgical dressing myself, having entirely limited my practice to that of a pure physician, but as a disinfectant it appears to

me so plain, and the benefit likely to arise to surgery from its use so great, that I earnestly hope my suggestions may meet with a favourable reception on the part of the profession, and that they may eventually lead to its adoption by those whose privilege it is, from the position they occupy, to put new discoveries to the test, and who—what is of more importance still—from the authority with which they are invested, have every facility to introduce them into practice.—*Medical Times and Gazette, May* 11, 1878, *p.* 518.

11.—SALICYLIC ACID AS AN ANTISEPTIC AND AN ANTIPYRETIC.

By Engledue Pridreaux, Esq.

Up to the present time the antiseptic system of treatment as applied to the zymotic diseases has failed, more or less, for the reasons that either the drugs used have upon absorption into the system been so changed that their properties have altogether departed, or they have exercised such a poisonous influence upon the system that their administration has either been impossible, or being attended with danger and disagreeable results, it has been in such small quantities as to be practically of little value in the treatment of the disease.

With salicylic acid, however, the case is different, its administration when pure in large doses is unattended with danger, for in one or two cases where unlooked-for complications have occurred after its administration, it is more than doubtful that they had anything to do with the exhibition of the drug, both from their nature and from the fact of these complications being unique in its administration. I have fed a Guineapig with biscuit containing salicylic acid so that it consumed 20 grains daily for some days without any apparent detriment to it, and have administered to it doses of 10 grains at a time with the same result, except to give it a laryngeal catarrh and hoarseness of voice, which I more than suspect was due to some of the acid "getting down the wrong way." I have administered to patients ½ oz. in the course of twelve hours without any ill result.

That salicylic acid is a germicide has been proved by eminent observers, whose results I have confirmed by a series of experiments on various liquids. I find that urine containing ½ grain of salicylic acid to 1,000 grains of urine, of the specific gravity 1,025, and at a temperature of 60° F. remains free from smell and cloudiness for fourteen days. Bacteria in small numbers are developed after three days, but their activity is not great. With one grain of the acid a similar result occurs, the number of bacteria is less, they are more slowly developed and mostly

confined to the upper stratum of the liquid, whilst with 1½ grains to 1,000 of urine, the urine remains clear, free from smell and bacteria for an indefinite time. A slight growth of fungous spores usually takes place on the surface after some days, if freely exposed to the air, and frequently in certain specimens of urine a crystalline sediment of salicylicuric acid forms about the sides of the glass. This is frequently to be observed in the urine of patients taking the acid. In urine to which salicylicate of soda is added the same antiseptic effects are produced, but the quantity of the salt requires to be proportionately larger, about ¼ more to produce an equal effect with the acid, but the growth of the fungi is much freer in these cases. Ordinary urine becomes turbid, has a putrid smell, and contains crowds of active bacteria in three days. These effects produced experimentally are to be observed with regard to the urine of patients taking the drug. It never decomposes and seems to keep pure for an indefinite time.

A much larger proportion of the acid is required to sterilize a liquid already containing bacteria. If one grain of salicylic acid be added to 1,000 grains of putrid urine after twenty-four hours a large number of the bacteria are dead, and the motion of the rest is sensibly diminished when compared with those in a portion of the original fluid. To kill all the bacteria in a putrid solution of urine requires from 3 to 4 grains per 1,000 of urine. The result of these experiments would seem to show either that the suppositious germs from which these bacteria spring are more easily affected by the acid than the bacteria themselves, or that the smaller quantity required to prevent their development is not sufficient to destroy them when developed. It was noticed that although in urine containing as little as ½ grain of the acid per 1,000, no smell was developed after twenty days exposure, whereas when 4 grains of the acid was added to 1,000 grains of putrid urine, although after thirty-six hours all the bacteria were dead, the smell was hardly affected.

A solution of albumen requires one grain of salicylic acid per 1,000 to prevent the development of bacteria for twelve days, while it requires 2 grains to absolutely sterilize it.

Milk requires more salicylic acid than either albumen or urine. As much as 3 grains per 1,000 of milk being required to absolutely sterilize it, although the acid delays the curdling it does not prevent it. The curdling that ensues appears to be due to the action of the acid itself, and not to the decomposition of the milk from the formation of bacteria.

In all the experiments upon milk, fungi were freely produced upon the surface, and it is evident that a much larger proportion of the acid must be used to prevent this growth.

When salicylicate of soda is added to blood it prevents it from coagulating, at the same time keeping it pure and undecomposed when added in weak solutions to small quantities of blood. Under the microscope it appears to exert no influence upon the red-blood corpuscles—after some hours they appear unchanged. I am unable to state with absolute certainty that it exerts an influence in lessening the power of motion of the white-blood corpuscles, but as the result of a few experiments with a view to ascertain that fact it does appear to do so.

The result then of the action of the drug and its salts upon the fluids outside of the body being these—viz., that it is a powerful antiseptic (that is it prevents the growth and further development of septic organisms, and also is very fatal to all the lower forms of animal life), and it possesses this property in the highest degree,—then should we not expect a somewhat similar action when administered as a drug, provided it be not so changed in the body as to affect its properties.

When pure salicylic acid is administered, part of it passes down the intestinal canal unchanged and may be detected in the fœces, whilst the rest is absorbed into the blood: apparently in the form of the neutral salts of soda and potash, it is then eliminated by the kidneys and appears in the urine in the form of salicylicuric acid. I have, when taken in solution as one of its salts on a nearly empty stomach, detected it in the urine in ten minutes, and in the blood in less than that time.

Now it appeared to me that if the theory of the zymotic diseases being due to the presence and multiplication of germs, or morbific ferments or organisms of any kind was true, then in salicylic acid we had a drug (which being equally powerful in its action after absorption into the body) with which we could cope with these diseases at their first onset, reducing at least their severity if not cutting them short altogether, and with this view I treated at Derby in 1876 all the cases of small-pox which came into the hospital. The results were very satisfactory as far as they went, (see Lancet July 28th, 1877), there being no deaths out of eighty-eight confluent cases, and the subsequent pitting was absent, but the number of cases was too small for any decided result. Since then I have treated in a similar manner twenty-eight cases of scarlet fever, some of which were very severe, with only one death and that was a case where I was called at the height of the disease. I have treated a large number of cases of measles and a few cases of typhoid-fever with the same drug without any deaths. The primary effect of its administration is almost invariably without exception to reduce the temperature rapidly, and with a considerable fall if the doses be large and sustained to very often below the normal standard, and this is observable not only in the zymotic fevers

but in all cases of disease with high temperature, including pneumonia, phthisis and acute rheumatism. In small-pox, I have notes of cases where a fall from 106° to 101° has taken place after the hourly administration of large doses for a few hours. In typhoid fever from 105° to 96° in twelve hours. In scarlet-fever from 103° to 98°, and in acute rheumatism numerous cases where the temperature has fallen from 104° to 98·4° in twenty-four hours, and from 104° to 100° in six hours, and under the administration of the drug the temperature keeps down, so that even if the drug does not exert a germicidal action it keeps the temperature down, preventing the excessive tissue waste and loss of power attending pyrexia, thereby enabling the organs to eliminate the poison.

How is this antipyretic action of salicylic acid to be explained? In a work entitled Disease Germs, and in the Lumleian Lectures for 1875, Dr. Lionel Beale states as a result of microscopical investigation, that the blood in conditions of inflammation contains quantities of minute particles of protoplasm or bioplasm possessing motion, and the power of self increase in prodigious numbers, and he attributes the condition of pyrexia to the consequence of the increase of the bioplasm or living matter, whether it be simple or contagious and morbific, and also as the results of his researches upon the cattle plague published in the Reports to the Privy Council, he clearly demonstrates an enormous increase of germinal particles in the blood and tissues in the contagious disease. Taking these facts into consideration, is it not probable that the action of salicylic acid in destroying the lower forms of animal life may be, and is of use in restraining the activity and increase of the bioplasmic particles which appear to be the accompaniment if not the cause of pyrexia, as well as in destroying their morbific and contagious influence; that is to say, as it destroys low animal organisms out of the body, may it not do the same in the body, for we must regard these shapeless masses of protoplasm as analogous to the lower forms of animal life.

It need not be argued that if salicylic acid will destroy morbid products in the blood that it must so alter its essential elements as to injuriously affect the constitution; that it does not do so we know from experience, and it must be remembered morbid products are as a rule more prone to change, more easily affected, and have a lower vitality than the natural structures, a drug may easily be conceived to destroy the one without affecting the other.

As regards the specific contagious disorders, the antiseptic system of treatment, founded on the theory of their germ origin, was advocated by Dr. Sansom, who used sulpho-carbolic acid and its salts with very good results a few years ago; but

salicylic acid is so superior to these not only in its greater activity, but in the ease and safety with which it can be administered in sufficient doses, that it affords a means by which a much more thorough trial can be made of the antiseptic system of treatment as applied to the contagious diseases.

The results of the treatment of acute rheumatism by salicylic acid are now almost fully determined, and from the great amount of recorded and accumulated experience of its action, its beneficial effect may be considered as proved. From an experience of about forty cases I find that it cuts short the disease, so that it now lasts as many days as it did weeks formerly; it quickly reduces the temperature and keeps it down whilst the poison is eliminated from the system, if it does not have a specific action upon the disease itself, and by thus lessening the duration and intensity of the disease it lessens the probabilities of complications, more to be feared than the disease itself; and this it does in a way which cannot be hoped for by the use of any other remedy. It is true that a few failures have occurred in its use in the treatment of this disease, but in the larger number of these failure has evidently been due to timidity in dosing and in administering it frequently enough, for the drug is so rapidly eliminated from the system that unless its effects are kept up by hourly or very frequent doses for some time it is not of much avail.

I have spoken of the safety with which it can be administered. Certain inconveniences have occurred in its use (more frequent formerly than of late) alarming in their nature and severity, these I believe to have been due to the method of exhibiting it and to the impurity of the drugs—these symptoms are sudden and rapid collapse, and cerebral symptoms varying from effects similar to those of cinchonism to almost acute delirium. The condition of collapse I have found to supervene after the exhibition of large and continued doses. This I have believed to be due to the rapid fall of temperature, and have found it to be obviated by giving salicylicate of ammonia. I prefer to produce a combination of salicylicate of soda and ammonia as in the following prescription:—Sodæ bicarb. gr. v.; ammon. carb. gr. v.; acid salicylic gr. xx.; aq. ad. ℥i.—*Practitioner*, *Sept.* 1878, *p.* 177.

12.—TREATMENT OF ACUTE RHEUMATISM BY SALICYLATE OF SODA.

By Dr. Seymour John Sharkey, Resident Assistant Physician to St. Thomas's Hospital.

Some cases were treated with salicin, some with salicylic acid, but in the great majority salicylate of soda was employed; for the latter not only has the advantage of being

readily soluble in water, but it seems also to be more effectual than the other two. At first 30 grains every two or three hours was the quantity prescribed; but latterly 20 grains repeated at the same intervals have been found to answer the purpose equally well, and to be capable of being taken with less chance of unpleasant results. Smaller quantities than these, however, are rarely effectual in the adult.

When the drug is given in these quantities the first result usually is diminution of pain, and so rapid is this that it often follows the first or second dose. With it the temperature also is reduced, and there is profuse perspiration. The patient generally complains too of deafness and noises in the ears. These effects are pretty constant, and may be accompanied by nausea or even vomiting, so that the medicine has to be stopped; the latter, however, is rather an exceptional occurrence. The swelling and redness leave the joints much less rapidly than the pain, and the tongue often remains furred long after the patient feels quite comfortable, and is almost free from fever.

Salicin has the advantage of producing to a far less extent, and often not at all, the unpleasant phenomena which are pretty constant when salicylate of soda is given: but its power of reducing the temperature seems to be much smaller than that of the salicylate, even when given in the same quantities.

A not at all uncommon accompaniment of the internal use of salicylate of soda is a profuse miliary eruption, which very often becomes pustular. Sometimes vesicles of a considerable size, filled with pus, are distributed over the body, and even a succession of very troublesome pustules may result. The greater frequency of the miliary eruption when this treatment is employed, and its greater proneness to suppuration make it probable that it is in some way due to the salicylate, especially when we remember what profuse diaphoresis is produced by the drug. In one case a general erythema preceded the miliary eruption, and in another urticaria occurred, but I have only seen one case of each.

In some cases the drug seems to affect the nervous system, more especially, and delirium may be very rapidly produced. In other cases nervous symptoms do not supervene until a considerable quantity of the salicylate has been taken. Rapidly supervening delirium is not so common as that produced after a while, and when it does occur it is often exceedingly violent. A curious circumstance is that if in such a case the medicine be stopped until the delirium has passed off, and be then again administered, the patient sometimes takes it without any recurrence of cerebral symptoms.

The delirium which occurs after a considerable quantity of the drug has been taken may, or may not, be violent; it is

generally preceded and accompanied by great restlessness, rapid breathing, and dryness of the tongue. The patient dreams, and has varied hallucinations. One patient, for instance, thought he had left the hospital and had gone to a theatre, where he saw duelling going on and people advancing to kill him, and the entrance to the ward appeared like a lighted tunnel. Still, however unpleasant the immediate consequences of the administration of the drug to patients who take it badly, these all rapidly subside after it is left off, and no permanent injury is done.

It is at present impossible to distinguish those cases who are likely to take the medicine with rapidly good effect, and without any unpleasant results from those who are intolerant of it. But it may be stated that persons in great pain, and with high fever, and in whom there is not, when the treatment is commenced, any complication, are, as a rule, the most favourable cases for it. Still slight complications, whether cardiac or pulmonary, should not preclude the treatment by salicylate of soda. Indeed, cases occur in which the drug produces rapid relief of the pain and joint affection, and no unpleasant symptoms whatever, notwithstanding the presence of pretty serious complications. Usually, however, the drug seems to have very little effect in modifying the course of cardiac or pulmonary affections occurring in acute rheumatism, although it may reduce the temperature in spite of them.

A girl of eighteen, for instance, had been in the hospital under Dr. Murchison for five days, with a temperature which ranged generally from 101° to 103°. Salicylate of soda was then given, which reduced the temperature to 97·8° in about fourteen hours. The medicine was then stopped, and on the third day from that time an acute pneumonia of the right lung, pericarditis, and pleurisy on the left side made their appearance. Twelve hours after these complications were discovered the temperature was still 98·6°, and in twelve hours more the girl died with her temperature below 100°, but with a pustular miliary eruption, pericarditis, left pleurisy, and consolidation of a large part of the right lung.

When the treatment is commenced before any secondary affections have made their appearance, the probabilities of their doing so are, of course, very greatly diminished, but they are not even under such circumstances necessarily prevented, for both cardiac and pulmonary diseases have arisen in several cases while the system was saturated with salicylate of soda.

The question of the production of albuminuria by the salicylate is one which has received some attention, and it has even been suggested that the delirium may be due to this cause.

There is no doubt that occasionally albumen makes its appearance in the urine after the treatment has been commenced; but the facts—that in the great majority of cases it does not do so; that in many cases in which albumen is present before the drug is given it disappears during its administration; and, finally, that albumen is frequently present in the urine of rheumatic patients with high temperature before any treatment whatever has been applied, go far to prove that the albuminuria ordinarily has nothing to do with the medicine.

It is only recently that I have been carefully observing the occurrence of albuminuria in cases of rheumatic fever with a view to determine how far it is a result of the treatment by salicylate of soda. Out of ten cases in which albumen was present, seven had it before the drug was administered, and it disappeared in all these while the urine still gave a strong reaction with the perchloride of iron. In the eighth case the urine was not tested for albumen before the medicine was given, but albumen was found in it and disappeared from it while it still contained the salicylate. In the remaining two cases the urine contained no albumen before the treatment was commenced, but it made its appearance afterwards, and again disappeared while the urine still contained the drug.

At any rate it can be stated with certainty that salicylate of soda, when given in the ordinary doses, never produces permanent albuminuria. That the delirium is not due to albuminuria is equally certain, for in many cases, if not in most, there is no albumen present during the period of delirium. It seems indeed, probable from the experience of this hospital that the presence of a small quantity of albumen in the urine should be no objection to the treatment by salicylate of soda, as the latter is just as effectual in such cases, and is not more likely to be attended with unpleasant results than in those in which the urine is free from albumen.

The delirium is probably due to the action of the salicylic acid on the brain itself. Of nine cases of delirium occurring during the administration of the drug, of which I have notes, two had neither albuminuria nor complications; two had a very small amount of albumen in the urine and no complications; two had no albuminuria, but had complications; two had no albuminuria, and the presence of complications at the time of the delirium was doubtful, while the remaining one had both albuminuria and complications.

It is, in fact, at present impossible to say in what class of cases delirium does occur, so varied is the condition of patients affected by it.

The liability to relapse after the salicylic treatment is considerable, especially when the drug is suddenly stopped. But

if it be continued in much smaller quantities for some time after the temperature is normal, this liability is very greatly diminished.

A very curious train of symptoms occurred in three cases of acute rheumatism, which were being treated with salicylate of soda, viz., a very high temperature, accompanied by great restlessness and delirium. In one case these occurred immediately after stopping the drug, and in the other two while the patients were still taking it. In one of these cases there was a considerable quantity of albumen in the urine before the treatment was commenced, but no other complications; in another there was no albuminuria, but a mitral systolic murmur; and in the last there was a trace of albumen, but no other complications. The only character which these cases had in common besides those mentioned above was the presence of a profuse miliary eruption, which became pustular. In one case the temperature rose to 106·4°, in the second to 106°, and in the third to 105·4°. In each case a graduated cold bath was given, which not only reduced the temperature to normal (though it rose again to a considerable height) but also put a stop to the delirium, restlessness, and insomnia. All three cases got well pretty rapidly afterwards.

As regards the effect of salicylate of soda on the amount of urine passed, I have not been able to come to any definite conclusion. It seems, however, often to diminish it considerably, and also the total quantity of urea. The percentage of urea, too, is affected in the same way, but to a much smaller extent.

Any one who has seen many cases of acute rheumatism treated by salicylate of soda must, I think, allow that its discovery as a cure for that disease is a triumph of empirical therapeutics, which has probably had but few parallels in the history of medicine. It has now had a fair and extensive trial, and to say that it far excels any other method of treatment would be to give the drug but scanty praise. It may rather be said that until the application of salicin and its compounds to the treatment of rheumatic fever, there was no drug which could be relied upon to shorten, to any great extent, its tedious course. Now, however, making due allowance for cases of failure, which do undoubtedly occur, not only can cessation of the primary phenomena of the disease—pain and fever—be rapidly secured, but we likewise have good grounds for hope, that owing to the remarkable power which the drug possesses of curtailing the duration of the disease, those secondary affections of the heart which make acute rheumatism so serious, may be greatly diminished in number and intensity.—*St. Thomas's Hospital Reports*, 1878, *p.* 75.

DISEASES OF THE NERVOUS SYSTEM.

13.—ON SOME POINTS IN THE DIAGNOSIS OF SPINAL SCLEROSIS.

By Dr. THOMAS BUZZARD, Physician to the National Hospital for the Paralysed and Epileptic.

If a healthy man sits with one knee-joint resting upon the other (the very common attitude), and the ligamentum patellæ of the supported leg be smartly struck just below the knee-cap with the side of the hand, a sudden contraction takes place of the quadriceps femoris muscle (of which the ligamentum patellæ represents the tendon), and the foot is consequently jerked upwards in a degree which varies in different individuals. Now in confirmed examples of locomotor ataxia this reaction does not take place. No matter on what part of the ligament below the knee-cap, or with what force the blow is struck, the foot hangs motionless. In order to establish with accuracy the absence of the phenomenon certain precautions ought to be taken. The leg should be bare; the patient must not offer voluntary resistance to the movement of his leg, and the ligament should be struck with some hard implement which can be swung like a hammer. I find an ordinary wooden stethoscope answer very well if it is held loosely by the small end, and the blow given with the edge of the ear-piece. But, however administered, several blows should be struck on the ligament, slightly changing the position each time, as there is generally one spot from which the reaction is peculiarly energetic. This is usually a little below, but very near to, the patella. Deferring for after-consideration any attempt to explain the phenomenon, I will describe how we found the test of importance in the diagnosis of a case now in the hospital. Careful and complete notes of the two patients referred to have been taken by Mr. A. E. Broster, from whose account I will, however, only glean sufficient to illustrate the character of the affection from which each is suffering.

M., a hairdresser, aged fifty, has been in the hospital since April. He complains of numbness, which is spread generally over the whole of his body and extremities, including his mouth and tongue. A day never passes without pains "like electricity," as he says, occurring in his left side and lower extremities, especially in the shins. There are, besides, sensory troubles of various kinds, burning sensations, aching of epigastrium, "cold water running down the legs," a feeling of tightness over the lower part of the belly as if his intestines were pushed out against the abdominal wall; this tightness is greatest when he stands up to walk, and it then seems to him to "force his motions and urine." There is great loss of muscular sense in

the arms and legs. In picking up a pencil from the table with his eyes open much irregular movement is shown, no matter which arm is employed; and when his eyes are shut, and his hands placed upon the object, he clutches it repeatedly, but is unable to take it up, and says that he cannot feel it. The soles of his feet are so anæsthetic that he feels neither the carpet, the strongest faradaic currents, nor the prick of a pin; and this condition is observed, though to a much less extent, over the body generally, the deficiency, however, being but slightly, if at all, marked in the face. There is occasional diplopia. The right optic disc is atrophied, the left unduly pale. In sitting, if his eyes are closed, he tends to sway about on his seat. In rising from a chair he is obliged to help himself up by resting his hands upon his thighs, and even with his eyes open he cannot stand steadily. When they are shut, however, he reels and pitches, so that he would fall helplessly if not caught. His gait is extremely ataxic, the steps short, the legs far apart, and kept stiff at the knee joints. The muscular power is very considerable. His sphincters are but slightly under control. I have said enough probably to show that this is a case of sclerosis of the posterior columns. There is, indeed, no symptom wanting to form the picture of a typical case of locomotor ataxia, which the history (omitted for the sake of brevity) shows to be progressive in character. Not only are there the electric shock-like pains and the difficulty of gait intensified by closing the eyes, but there are also the cephalic symptoms in the form of atrophy of the optic disc, and occasional diplopia. I insist rather upon these latter symptoms, because, as regards the former (pains and gait disturbed by closure of the eye) they are liable to exist alone, and, if not very carefully examined, to lead to error. Indeed, in the case which follows an erroneous diagnosis was actually made from a superficial examination having caused undue prominence to be given to these symptoms.

The patient B., aged forty-three, came to me at the hospital last December on account of "paralysis." He walked very unsteadily with his legs wide apart, and could scarcely walk at all with his eyes closed. He stood upright with difficulty and uncertainty, and the effect of closing his eyes in increasing his insecurity was very marked. There was considerable loss of sensibility in the soles of his feet and legs, a sensation of a tight band around his waist, and a constant feeling of weight in his belly. He complained, too, of pains in his legs, and said that sometimes when walking sudden pains would shoot through his knee-joints, and nearly throw him down. In the pressure of work, the case, from these data, was hastily assumed to be one of posterior sclerosis, and a note to that effect was recorded on the patient's paper. It was not till some little time afterwards

character. This person not only exhibits no sign of muscular inco-ordination, but his delight is in mountain-climbing. He presents, indeed, none of the symptoms ordinarily attributable to posterior sclerosis, except the tell-tale pains. In his case, again, examination shows a total absence of Westphal's leg phenomenon. This is the only example I have at present met with of the test being applied where the indications of tabes dorsalis were absolutely limited to the occurrence of pains. In another case under my observation, in the person of a gentleman over seventy years of age, the pains are precisely of similar character; the gait is, besides, somewhat ataxic. Here equally it is to be noted there is no response to blows upon the patellar tendon.

Two questions naturally arise. What is the cause of the leg phenomenon? Why is it absent in posterior sclerosis? I do not think we are as yet in a position to give a positive answer to either, and the subject is still under investigation. One's first idea is that the contraction of the muscle must be brought about by a reflex from the skin at the part which is struck. But Westphal has practically disposed of this explanation. He pinched and pricked and irritated the integument in various ways without effect, even submitting a fold of the skin, lifted away from the tendon, to smart blows with a hammer. On the other hand, when the skin lying over the ligamentum patellæ was frozen by Richardson's process the effect of blows upon this spot in determining the contraction was in no way lessened. So, also, in cases (not being examples of locomotor ataxia) where there was complete cutaneous anæsthesia, the "phenomenon" has been found present. The influence of the skin must therefore be excluded.

The researches of Westphal, supplemented and carried a stage further by some interesting experiments recently conducted by Tschirjew, of St. Petersburg, appear to render it probable that the phenomenon is of partly mechanical and partly reflex origin. Tschirjew found that section of the cord in guinea-pigs above the place of entrance of the sixth lumbar nerve roots caused the phenomenon to disappear. In addition to this, he discovered that when the posterior root of one of the sixth lumbar nerves was divided the phenomenon failed on the corresponding side. If these experiments were free from fallacy, the reflex character would seem to be placed beyond doubt. It would appear, indeed, probable that the blow upon the patellar tendon, by suddenly stretching it, irritates some nervous fibres belonging to the tendon, the impression thus produced being conveyed to the cord, and there exciting the motor nerves to the muscles. Degeneration of the cord at the point where the nerves enter may easily be supposed to be capable of interfering

with the orderly reflex which thus occurs in health, and in this way the effect of posterior sclerosis in preventing the exhibition of patellar tendon reflex may possibly be explained. The very rare circumstance of the lumbar portion of the cord escaping the degenerative changes, in tabes dorsalis, would thus sufficiently account for the certainty with which, apparently, this diagnostic sign may be looked for. — *Lancet, July* 27, 1878, *p.* 111.

14.—TREATMENT OF HEAT-APOPLEXY WITH ERGOT.

By ROBERT F. DEDRICKSON, Esq., L.R.C.P. Edin.

When in Calcutta in July last, the heat was very intense, being one day 103° in the shade. It fell to my lot to have several cases of sunstroke under my care; and being struck with the great mortality arising from the disease, I am induced to lay before the profession the treatment I successfully adopted, believing it to be novel.

The first case I was called on to attend was that of a steward on board one of the Peninsular and Oriental Company's steamers. He was lying in a state bordering on coma, but was capable of being roused, and answered questions in an absent way. I obtained the following history from one of his companions:—Having been sent ashore about noon, he walked for nearly an hour in the heat of the sun, and, "feeling queer," indulged in some brandy and soda-water, On his return to the steamer, he complained of great pain in the head and all down the back. He had to lie down; and then it was that I was sent for, and found him in the following condition:—Pulse very rapid, strong, and bounding—almost sledge-hammer; action of the heart very strong and slightly irregular, but not intermittent; the pupils widely dilated. The sensation given on placing the hand on any part of the body resembled exactly the feel of a board that had been exposed to the rays of a powerful sun, being burning hot and dry. I ordered him to have ice applied to the nape of the neck and head, and to have fifteen minims of liquid extract of ergot and three minims of tincture of aconite every hour. The bowels were loose, therefore I did not order a purgative. I saw him again the following morning, with Dr. Waller, of Calcutta, whom I had the pleasure of meeting in consultation. He was then greatly improved; pulse quiet, temperature reduced, not quite so drowsy, but still suffering from the pain down the back and in the head. The nurse who sat up with him during the night stated that when she gave him a dose of twenty grains of quinine—ordered him by Dr. Waller in the night—he vomited and appeared so bad that she gave him a dose of the mixture (ergot and aconite). He at

once (she said) appeared better, and improved after each succeeding dose. Dr. Waller and myself agreed to continue this treatment, and with the greatest success, as in a few days his recovery was complete.

I may here mention that the twenty grains of quinine given by direction of Dr. Waller is a very usual remedy in the East for sunstroke. The ergot treatment which I adopted was unknown to Dr. Waller, whose experience and practice are very great, but I have pleasure in knowing that he highly approved of it.

Shortly after the above case, I was called to another—that of an English sailor who went ashore, during the heat of the sun, wearing a small black hat, and, on his return to his ship, became quite insensible. When I saw him I had ice placed on the nape of the neck and let dissolve there. In about fifteen minutes, when he was sufficiently conscious to swallow, I gave him ergot—leaving out the aconite, as in his case the action of the heart was very weak. He also made an uninterrupted and good recovery.

I likewise treated several minor cases of heat-apoplexy—in which there was great pain down the back and in the head, with suppression of perspiration—successfully with ergot.

When attending these cases it struck me that had the state of coma advanced so far that the patient could not swallow the ergot, ergotine might have been usefully administered hypodermically. I offer this now as a suggestion only, as I do not know whether in a case so advanced it would be of any use, yet I think it would be worthy of trial.—*Dublin Journal of Medical Science, Oct.* 1878, *p.* 285.

15.—SANGUINEOUS CEREBRAL APOPLEXY TREATED BY SUBCUTANEOUS INJECTION OF ERGOTINE.

By Dr. N. S. Foster, Wisbeach.

The utility of the subcutaneous injection for the exhibition of the active principle of ergot on account of the rapidity and comparative certainty of its action has been most successfully demonstrated in cases of post-partum hemorrhage. From the explanation given of its inducing the contraction of the smaller arteries, and from the facility of its administration, especially in cases where swallowing is at least very difficult, I was led to use it in cases of cerebral apoplexy and also of hæmoptysis. It is for the former that I am enabled more especially to suggest its use, and from the results I have seen believe it worthy of a more extended trial in that form of disease.

Cerebral apoplexy proper, pathologically speaking, is essentially effusion of blood caused by a rupture, generally of the

smaller arteries of the brain, whether of the punctiform or of the massive varieties—which, indeed, are more accurately degrees of the same condition. Perhaps the commonest kind of disease leading to this result is the formation of minute miliary aneurisms, their subsequent rupture, and thence the usual train of symptoms.

At present I can record only two cases in which I followed out the plan of treatment.

Case 1.—I was sent for and informed that the patient, aged seventy-two, had been seized about half an hour before my arrival. The ordinary apoplectic symptoms were present, and the coma gradually deepened during the application of the usual remedies. I then injected ergotine subcutaneously in the fore-arm. The comatose state soon seemed to become stationary, and eventually the patient made a good recovery.

Case 2 was similar in most respects to No. 1; but in this patient, who was sixty-four years of age, I injected ergotine at once; and here the coma, which was only partial on my first seeing the patient, never increased in intensity, but soon passed off, and to all appearances he made a perfect recovery.

In both cases I satisfied myself of the absence of cardiac disease, and hence possibly of embolism; and from the history it was fair to conclude that effusion was the cause.

For the success of this treatment, both temporarily and permanently, a great deal depends on the promptitude of its administration, before much hemorrhage has taken place and consequent damage to the cerebral substance. The strength of the injection I employ is ten grains of ergotine to the fluid drachm; injecting twelve minims deeply into the muscles, and not merely into the subcutaneous tissue, as in the latter case suppuration is very apt to ensue.—*Lancet, Sept.* 21, 1878, *p.* 401.

16.—ON THE SUBCUTANEOUS INJECTION OF CURARA FOR CHOREA.

By Dr. R. T. WRIGHT, Surgeon, Bengal Army.

Attention having been drawn to curara by Mr. Alfred Simson and by Dr. D. Drummond, notes of the following case may be of interest.

Mr. S. C., aged seventy-eight, widower, farmer, of Hopton, Shropshire, a tenant of Viscount Hill, was an object of much local sympathy, on account of his suffering from true chorea for three years, which had resisted all sorts of treatment; so I was asked to see him in consultation. I visited him with Dr. John Lyon and Mr. W. Gill, son of Lord Hill's agent, and found him almost worn out by jactitations so constant and so violent that he was thrown off his bed, unless protected by

railings on every side. He could not walk, and his speech was quite unintelligible, except that a few monosyllables, such as "tea," "no," "here," were shouted out with a painful effort. His spirits were very low, he had great difficulty in swallowing, and he could scarcely ever fall asleep without a dose of chloral hydrate, of which he was very fond, as he was quiet only when asleep. Fortunately his appetite was very good and his bowels regular, though he had no teeth and he was always eating sweets. It seems his family is subject to chorea; several relations had it at various times of life, and his mother, who died at the age of eighty-five, is said to have suffered from chorea for several years before her death. He was so restless that it was impossible to ascertain his pulse, respiration, or temperature; his urine on examination was found to be clear, neutral, of sp. gr. 1010, free from albumen or casts, but it threw down a faint cloud of phosphates with heat, which readily dissolved with a little nitric acid.

As curara had been recommended in hydrophobia, I suggested it might be tried in chorea, so solutions of the following strength were injected subcutaneously on the dates specified. Jan. 21st, 1878, one-twentieth of a grain; Jan. 22nd, one-twentieth of a grain; Jan. 26th, one-tenth of a grain; Jan. 28th, two-tenths of a grain; Jan. 31st, four-tenths of a grain.

On Feb. 2nd four-tenths of a grain of curara were injected. Pulse (felt for the first time) 68, regular. The jactitations were now far less violent, and both the patient and his relatives remarked the improvement without any bad effect. Four-tenths of a grain were injected on Feb. 4th, 5th, 6th, 7th, 8th, 9th, and 10th, by which date he was perfectly quiet, able to walk without help, and could sleep at night without chloral. He had no other medicine at all but curara since the subcutaneous injection treatment was begun. Four-tenths of a grain were injected on the 14th and 18th; on the latter date he spoke a sentence, slowly, but plainly, and readily intelligible by listening attentively. On the 19th, four-tenths of a grain were injected, the patient being in remarkably good spirits. The same quantity was injected on the 22nd, on which day Mr. C. put on his spectacles and read his bible—the first time he had been able to do so for three years.

From the last date he has been kept quiet by injecting four-tenths of a grain of curara twice a week. His speech is still of abnormal character; but he will not submit to the operation more than twice a week, as he has a morbid dread of the pain, and often breaks the nozzle of the syringe by his frantic struggles when the doctor approaches him. His relatives say he always, even when in health, had a morbid dread of pain, never would wash in cold water, and howled piteously at the slightest

scratch. He was sent to Matlock at the outset of his treatment to have proper shower-baths, but he would not stay a week, as he dreaded the water.

The preparation used in Mr. C.'s case was by Messrs. Savory and Moore, in the form of discs of gelatine, a quarter of an inch square. It is made of three strengths, in which each disc contains respectively one-hundredth of a grain, one-twentieth of a grain, and one-tenth of a grain, so that it is easy to obtain a solution of any desired strength. A few drops of water are placed in a spoon; then the discs required for use are added, and the fluid is warmed over a lighted taper; stirring, if necessary, to assist solution. It is necessary to clean the syringe with warm water directly after use, as curara is very sticky, and solidifies when cold so rapidly that it is apt to block up the nozzle of the instrument.—*Lancet, August* 24, 1878, *p.* 253.

17.—ON HEADACHE.

By Dr. E. Symes Thompson, Gresham Professor of Physic, &c.

Headache is merely the name of a symptom which may occur in a multitude of disorders. It is often met with in the course of jaundice or kidney disease, and is then regarded as part and parcel of the deeper underlying ailment. The term, however, is employed mainly to describe those conditions in which head-pain is the pronounced and manifest evil.

The better to understand the nature and varieties of headache, some description of the parts involved is needed. This anatomical description must be of the most general kind. Beginning from the outside, there is first the hair, next the scalp, which covers the bone, next the bone covered by periosteum, and next the brain covered in its turn by the dura mater, a fibrous tunic which embraces and keeps it together, and sends partitions between the hemispheres and between the larger (cerebrum) and smaller (cerebellum) brain. The dura mater supports the vessels which convey blood to and from the brain. This rapid survey sufficiently indicates that, as the parts affected are various, so the painful sensations to which these parts are subject, may be various in kind, in degree, and in results.

Headache, like any other pain, is given as a warning, not to be slighted but to be attended to, and very often it is the earliest evidence of removable mischief, which, if neglected, passes into hopeless disorganisation.

The scalp may become tender and painful from hard brushing, parting the hair in an unusual place, or hanging on to it a weighty superstructure. Those who have too much hair or wear it too long have headache sometimes in consequence, and persons leading a sedentary life with excess of food and wine, find

the tonsure promote coolness of head and freedom from oppression. The malady as it affects gouty subjects is often accompanied by heat of head, and it is a good thing for gouty old gentlemen that they are often bald. *Gouty* headache is accompanied by fulness, flushing of the vessels, and if neglected may lead to giddiness and apoplexy.

Rheumatic headache attacks those who are not comfortably housed and well supplied with warming food; it favours persons living in damp low-lying situations or exposed to raw winds when imperfectly clothed; it fixes itself in one part of the aponeurosis between the scalp and skull; and notwithstanding warmth, anodyne embrocations, and alkaline draughts, it is very apt to remain for several days, and only yields to iodide of potassium and sarsaparilla.

This differs from *Neuralgic* headache which occurs in plunging paroxysms like toothache. Often it depends on a bad tooth and is removed with it, or it is intermittent and dependent on ague or marsh poison, in which case it may be cured by a good dose of quinine. Sometimes intense neuralgic headache depends on a swelling on the nerve as it passes a foramen or hole in the skull, and then mercury or iodide of potassium must be relied upon.

Nervous headache, as it is called, is not like neuralgia. To depict a case: here is a pale, thin woman, with bright eyes and an anxious over-wrought expression, who tells us she is a martyr to it; sometimes she is free for weeks, but when she is "put about" or worried, the head is almost constantly aching, unfitting her for work and making life a burden. It cannot, she tells us, be due to over-eating, for she takes little besides tea and toast, she never takes any breakfast, and the pain is worse in the morning. The pain is, indeed, "the prayer of the nerve for healthy blood." On inquiring the cause, we find that it began first years ago, when her nights were disturbed by a sick child and by her husband's misfortunes.

Cases of this kind are common among seamstresses and underpaid washerwomen, but they are not rare in the higher walks of life, for the ladies who look so graceful and prosperous in their luxurious carriages are often harassed by anxieties we know not of, and tormented by carking cares to which the poor are strangers. These are cases in which valerianite of zinc is useful; the attack may be lessened by *guarana*, the new popular tea-like drug; but to supply the worn nerve with good blood is the real point, and food containing fat is essential, as milk and cod-liver oil, and let bread and milk take the place of tea. Although wine or spirit *taken with food* may be of real temporary benefit, it is the doctor's duty in cases like this to discourage the use of alcohol, for soon a craving for it will be developed,

which, from weakened powers of resistance, the patient will be unable to escape.

The very opposite conditions to those just described give rise often to headache. I mean not defective or lacking nourishment but over-eating. Bad cooking or imperfect mastication *may* be the cause, but generally eating too fast or eating too much is the evil. Some may remember my allusion in this hall years ago to the evil of luncheon bars. The observations were quoted in some of the commercial papers of this city, and I hope have been taken to heart by some; there can be no question that many become dyspeptic and get headache because they allow themselves but a few (five or ten) minutes to "bolt" (no other term is appropriate) a plate of meat, with beer and perhaps cheese and salad, and are at their desk and bustling work without a moment's interval for the stomach to attack the food while the brain is untaxed. If the time is very short it is better to take a light meal than to "bolt" a heavy one.

In this dyspeptic form of headache the feet get cold, the head hot, and face flushed; soon a dull heavy pain comes with throbbing at the temples. The tongue becomes coated, mouth clammy, breath offensive. The pain shifts about, and is increased in the upright posture. The patient declares, perhaps, that the headache cannot be due to indiscretion in diet, for he had a specially light, wholesome meal the previous day; on inquiry, it will be found to be due to an error two or three days before, or most probably a wrong plan long followed.

Headache from over-eating in children, may be relieved by an emetic. Growing children *must* have their digestion in order, or they will be stunted in growth, and imperfect in development.

Headache from over-drinking, comes on the morning after the "bout," and may be unaccompanied by any sign of disorder of digestion, for the patient endurance of the stomach is almost beyond conception. It is relieved by brandy and soda, or better by soda water, or even by cold water alone.

Let me now bring before you a typical case of plethoric or congestive headache. Here is a burly, fresh-coloured gentleman, who looks the "picture of good nature," and whose face certainly does not pity him; but if you look at him attentively, you see the red cheeks are due to tortuous vessels and stagnant circulation. His lips, too, are blueish, and the nose red. When he stoops he flushes deeply. He complains of noises in the ears, and pain in the bridge of the nose; and he gets so sleepy after meals that he *must* have his nap. His habits are sedentary, and he becomes less and less disposed for walking. He has, perhaps, lately retired from business, having made his fortune. He considers his diet spare, as he seldom eats meat more than

three times a day, and rarely takes anything between lunch and dinner, and he is "doing Banting" to the extent of taking a biscuit with his cheese and port. Two or three times he has had nose bleeding. This may probably have saved him from apoplexy, and till he gives up his port, and two of the three heavy meat meals, and leads an active life, he will suffer from his headache, and perhaps some morning will have an apoplectic seizure and never regain consciousness.

Time will not allow me to speak of the headache of hysteria or hypochondriacs.

Sick headache is primarily due to nerve wear, but being treated by anti-dyspeptic remedies, it becomes accompanied by, and eventually chiefly caused by, stomach disorder. It is apt to attack those who inherit a predisposition to affection of the brain. The grandmother of the patient was perhaps epileptic, father subject to tic, one brother consumptive, and another insane. It is periodic in its onset, necessitates complete recumbency and withdrawal from noise, glare, and bustle. The sickness is the consequence, and not the cause of, the headache, just as sea-sickness is dependent on disturbed circulation in the brain, secondarily affecting the stomach. In treatment, many things, *e.g.*, sal. ammoniac, mindererus, coffee, or chloral, give relief, but our sheet anchor is a prolonged course of iodide of potassium.

The headaches of children require special care and discrimination; those of measles and scarlet fever soon pass away on the appearance of the rash. They are at times as severe as is the characteristic backache of small-pox.

School-boys and students sometimes get a severe form of headache from overwork, which needs vigorous and decisive handling, or it is followed by organic disease. Several distressing cases of this kind have fallen under my care. In such cases cessation from head work is the first essential. Country life or a sea voyage are the remedies. These are cases in which the brain tissue is deteriorated and softened either from strumous disease or overstrain of feeble brain. In fevers the consistence of the brain is reduced, so that its specific gravity is less than it should be. In insanity it is harder and heavier, the fluids around being increased. The proportion of phosphates, too, varies considerably; hence the suggestion that phosphorus is the remedy in such cases.

Disease of brain is sometimes painless, for the brain substance has no sensitive nerves, and in hernia cerebri you can touch the brain without the knowledge of the patient; but if you press firmly on it, uneasiness or convulsion occurs.

If the membranes covering the brain are affected, there is sure to be pain, generally of an abiding kind, and in a fixed spot.

In such a case the pain is increased by engaging the patient in conversation, instead of being diminished, as is usual in other forms of headache.

A large abscess may destroy a hemisphere without symptoms, as in a case once under my care, but a small tumour on the surface may give rise to acute pain, and perhaps paralysis. Tumours are of many kinds, fibrous, cystic, aneurismal, or cancerous. This is a subject for a lecture in itself. I can only allude to it here. Tumours often cause epilepsy, as well as headache. If the tumour is aneurismal, it may burst into the brain, with an immediately fatal result.

The treatment of epilepsy has of late greatly improved, and at least, partial benefit may now be counted upon. Typical sick headaches are, as I have said, allied to epilepsy. They are commoner now in these days of high-pressure and competition than formerly, for disorders of the nervous system have taken the place of the disorders of the circulation which so frequently affected our easy-going free-living ancestors.

A few words now about treatment. First *preventive treatment*. Had this been more successful in times past many valuable lives might have been saved. Isaac Newton always found that when he worked at the theory of lunar irregularities it made his head ache, but it never ached when studying any other subject. He neglected the warning, and after resuming his intense application to this abstruse subject, the severe illness which ultimately cost him his life, commenced; and thus the world lost with Newton the power of mastering other phenomena as important perhaps as that of gravitation. The value of one such life who shall estimate!

Congestion of brain, when habitual, leads to feeble memory, dimmed intellect, weak sight, or perhaps even to blindness and deafness; a cautious bleeding (old-fashioned remedy though it be) may prove the best treatment in such a case.

Symptoms ought to be *early* attended to: work must not be too prolonged or intense, nor must the intervals between meals be too long, or the quantity taken unsuitable to the habits and requirements of the body. Hard mental work cannot be done without good sustaining food, any more than an engine can work without fuel. Let the diet be regulated; let the clothing be ample but not excessive; let the digestion be kept in order, the exercise be appropriate, the rooms airy.

These are general principles applicable to all. But remember that headache is a symptom. Its cause must be discovered and obviated. When ascertained, if found to be dependent on rheumatism, a great effort should be made to secure a dry comfortable house, on a gravelly or sandy soil. Flannel should be worn near or next the skin, vigorous friction after the morning

bath, a diet nutritious but not such as to develop acidity, for many forms of rheumatic headache are promoted by sugar, by porter, and sweet wines. I need not repeat what I said about the character of rheumatic headache, that it is superficial, accompanied by tenderness increased on moving the scalp; nor need I allude further to its causes or the treatment, which is that of rheumatism generally.

The *congestive* or *plethoric* headache may be known by the sense of fulness and weight in the head, with occasional giddiness. The pain is throbbing, it comes and goes, it may be produced by stooping, sleeping with the head low, wearing a tight necktie (or "choker," as it may more appropriately be called), or even by tight stays. This form of headache is not confined to those who indulge in good living, but it affects those, too, who are pallid and bloodless, for persons with too little blood are even more prone to congestion than those with too much. You will all see how essential a sound diagnosis must be under such circumstances, for what may cure the full-blooded, will certainly greatly injure those whose blood supply is deficient. These cases may be benefited by derivation, *i.e.* attracting blood to other parts, as by warm baths, or mustard plasters, by cupping, or by vigorous friction. Suitable drugs are too manifold to name.

In *nervous* headache and headache from overwork, the grand desideratum is *rest* for the overtaxed organs. As with rheumatic subjects, those prone to *nervous* headache are like barometers, and feel every change of weather. The ordinary pressure of the atmosphere is 15 lb. to sq. inch, a fall of one inch (from 30 to 29) will take off $\frac{1}{4}$ lb. pressure on every inch of surface, or about 1,000 lbs. over the whole body. If you go to top of Mont-Blanc, you reduce pressure by one-half.

Humidity and dryness have an equally marked effect. In dry air, moisture is given off by the skin, and thus the amount of fluid in the body is reduced, but in damp air this evaporation is checked. Thus, many people who have headache in low-lying damp swamps get well in dry air.

The electrical conditions of the air tend to influence the feelings of sensitive invalids, and when a thunder storm is imminent, many people suffer from headache, which is relieved when the storm has passed off. Again, a cold, raw, north-east wind blowing in the face will often give headache either to a person subject to rheumatic, congestive, or nervous headache. Thus travelling, often so beneficial, may become a source of evil.

The palliative remedies in many such cases are numerous. Eau de cologne and camphor water are often comforting. Ice is refreshing, or iced seltzer water; smelling salts to the nose,

or snuff may be useful. A mixture of ether and aromatic vinegar is grateful at times.

In *dyspeptic* headache, the pain is at the back of the eye, with a throbbing at the temple on movement. Care in diet is the grand requirement. If due to acidity a simple antacid and carminative, as soda with ginger, or rhubarb and magnesia, will do great good. Sedatives are generally *bad*, they either fail to relieve, or stupify the patient.

In this lecture, I have felt it better to describe a few main varieties of headache clearly, so that you may realise the nature and treatment of each; although in practice each case may have something in common with two or more varieties. Thus a nervous dyspeptic may suffer from sick headache, or a full-blooded plethoric person from gouty and organic as well as congestive headache. We cannot treat every case even of the same variety of headache on the same plan; every case is a study in itself, and that practitioner is the most successful who best adapts broad principles to the personal idiosyncracies of the sick person before him.

Do not neglect a headache nor attempt to remove it by a dose of opium—*but find out its cause*—and *then* think no pains thrown away in removing it; few things are more easily dealt with in an early stage, and few maladies try the skill more when they have been long established. Much may be done by judgment and discretion, and in a large proportion of cases cure may be counted upon if the effort to attain it is proportionate to the importance of the end in view.

The *prognosis* must depend entirely on the *diagnosis*; if there is organic disease of the brain recovery cannot be counted upon; happily, however, such cases are quite exceptional. In the rheumatic, neuralgic, congestive, dyspeptic, and nervous varieties, cure should be determinedly sought and found. In sick headaches alleviation and diminution of frequency in the attacks is certain. In gouty metastatic headache the prognosis is less hopeful, and when the disorder depends on jaundice or deep-seated disease of distant organs, recovery from the headache must always be dependent on the course of the malady from which it springs.—*Medical Press and Circular*, *June* 19, 1878, *p.* 499.

18.—ON THE CAUSES AND CURE OF INSOMNIA.

By Dr. JAMES SAWYER, Physician to the Queen's Hospital, Birmingham.

[Perhaps the most common form of insomnia arises from an unnatural excitation of the cerebral cells. There are, however, other two forms, the toxic and the senile.]

In toxic insomnia, the cause of the sleeplessness acts primarily upon the vessels of the brain, giving rise to some degree of arterial hyperæmia. A poisonous agent maintains cerebral vascularity at such a height that conscious cerebral activity—that is, wakefulness—is an inevitable consequence. Such a poison may be introduced into the body from without, or it may be a product of diseased processes arising within the body itself. Of course I use the word "poison" in a restricted sense; I do not mean something which kills, but only something which produces abnormal manifestations in the living body. The external poisons which most frequently cause sleeplessness are tobacco, alcohol, tea, and coffee; the internal, certain effete products of tissue-metamorphosis which accumulate in the bodies of gouty persons or of those whose kidneys act deficiently. Many a man does not and cannot sleep sufficiently because he smokes excessively. Cut off his cavendish or his cigars and he will sleep well. Many smokers know that they sleep badly if they smoke more than their usual quantity of tobacco, or if they smoke stronger tobacco than that to which they are accustomed. If a man who smokes two cigars every evening is induced at some time to smoke three, or if a smoker of bird's-eye ventures to replace it by cavendish, he may, when he has gone to bed, find he cannot sleep; and the cause of his sleeplessness is the smoking of more or of stronger tobacco than by habit he has hitherto borne without discomfort. Men of distinctly nervous temperament, or men in whose temperament there is a distinct and considerable admixture of the nervous element, are usually the largest smokers. Men who are slow and calculating are rarely smokers; men whose activity is of an objective type are happy in rarely feeling the nervous unrest which tobacco calms. Tobacco-smoking stimulates the cerebral circulation; it disposes to a succession of pleasing ideas by inducing an easy flow of mental activity. But this stimulation of the blood-flow in the brain is sure, if pushed to undue limits, to induce cerebral vaso-motor debility or paralysis, and, as a consequence, persistent conscious thought. Sometimes, then, a man consults us for the relief of insomnia, and we find he is young, he has had no trouble, he takes plenty of food and exercise, and he does not overtax his brain. But he is an excessive smoker; he smokes morning, noon, and night, and he has gone on from the mildest tobacco to the strongest. He need not give up, or shorten, or change his work, and he surely does not need drugs; cut off or cut down his smoking, and he at once sleeps well. And so, *mutatis mutandis*, does alcohol cause sleeplessness. The man who drinks to commencing drunkenness mostly sleeps soundly, if not well. But many a so-called moderate drinker knows that he sleeps badly if he takes a little more than his

usual quantity of wine after dinner, or even his usual quantity of some unusual wine. Alcohol flushes and dilates the smaller blood-vessels, especially those of the brain; if such a condition be maintained sleep is disturbed or wanting. We have all seen the insomnia of delirium tremens: the patient cannot sleep because the lesser arteries of his brain are paralysed by alcohol, and sleepless cerebral activity is the inevitable consequence. Far short of what is usually called alcoholism, we often meet with cases of insomnia in which alcohol alone is the cause of the wakefulness. The patient may pride himself upon his moderate use of fermented stimulants, and he may be wholly ignorant of the cause of the sleeplessness for which he consults us. We fail to find any sufficient psychic cause for his insomnia; but if we take away or diminish his wine or his grog, or induce him to consume it before the evening, we find that he shortly begins to sleep well.

The effects of tea and coffee in causing wakefulness are well known. Some individuals are extremely susceptible to the action of these stimulants. We sometimes meet with persons, mostly women, who habitually drink enormous quantities of strong tea; such people are usually troubled with flatulent dyspepsia, and sleep badly, but they rarely suffer from serious insomnia.

On this occasion I can only mention those varieties of toxic insomnia which are apt to occur in gouty persons, or in those whose kidneys are failing, and which arise from the accumulation in the blood, in consequence of deficient excretion, of the products of tissue-metamorphosis. Insomnia of this kind is rarely complete. But the patient may complain that he sleeps very badly, that he lies awake for some hours and has great difficulty in getting off to sleep, that he is easily awakened and wakes frequently, and that he always dreams when he sleeps. In such a case we may find a pulse of high tension; the aortic second sound may be accentuated, and the first sound of the heart may be reduplicated at the apex. Where there is chronic renal disease, we may also find the direct physical evidences of the characteristic cardiac hypertrophy which accompanies chronic interstitial nephritis. I believe that insomnia in such cases is due to the maintenance of a state of high tension in the cerebral arteries. I wish to impress upon you that we find the clue to many cases of sleeplessness in the signs of the gouty diathesis or in the discovery of albuminuria.

Again, there is a senile form of insomnia. You may perhaps have observed amongst your friends that an exaggerated appreciation of the merits and value of early rising mostly increases as age advances. The sleeplessness from which many old persons suffer is mainly, if not entirely, the result of senile

degeneration of the smaller cerebral arteries. Those vessels are less elastic and less contractile than in health, and their weakened walls often lead to their permanent dilatation; they are physically unable to adapt themselves fully to the condition of relative arterial anæmia which is requisite for healthy sleep. The tendency of this condition of the blood-vessels of the brain to prevent or diminish sleep is probably to a great extent counteracted by the cardiac feebleness which so frequently and so fortunately coexists with the vascular changes.

In the treatment of insomnia we must often use soporifics. Of these the chief are chloral, opium, morphia, the bromides, Indian hemp, alcohol, and affusion with cold water. The successful treatment of a case of sleeplessness follows from the discovery of its cause. In the severer forms of psychic insomnia we must at once secure sleep by some efficient hypnotic. I prefer chloral. By its use alone we can often quickly cure acute insomnia depending upon some sudden mental shock or strain. A few nights of sound and sufficient sleep, artificially induced, will do more than anything else to restore to the brain the power of sleeping without aid from drugs. In the more chronic forms of psychic insomnia, where the sleeplessness or wakefulness usually depends upon prolonged worry or overwork, I employ chloral sparingly. It should only be used as a temporary remedy, when it is necessary that we should at once secure a fair amount of sleep. The patient ought never to be allowed to swallow this dangerous but valuable drug whenever he feels disposed, or to apportion its dose for himself; he ought only to take it upon the special prescription of his doctor. An overworked man must never be permitted to go on with his overwork, and habitually secure sleep by choral or any other hypnotic. In such a case we must always aim at preventing the sleeplessness by removing its cause, instead of permitting that cause to continue and trusting to counteract its effect by medicines. When a man cannot sleep because he works his brain too much, we must insist that he stop or greatly diminish his work. But I must warn you that real work is rarely the cause of insomnia. Work fits for rest. It is mostly worry, not work, that brings unrest. It is not work that wears, but worry. A holiday, with complete change of scene, will often do much to effect a cure. The old maxim, *Cœlum non animum mutant qui trans mare currunt*, like many other maxims, old and new, is not always wholly true. Send an overworked and worried merchant or barrister from his counting-house or his chambers in a busy town to a quiet village by the sea, or across the channel to a French watering-place, and let him substitute walking, and bathing, and rowing, and fishing, for his books or his briefs, and he will often need no physic to make him sleep soundly and

sufficiently. But many cases of psychic insomnia can only be cured with the aid of drugs. In the well-nourished, bromide of potassium is by far the best hypnotic. It soothes the irritated and irritable cerebral cells; it is a direct and absolutely safe brain sedative, and it is marvellously powerful in producing nervous calm. But it must be given in full doses, thirty to sixty grains at bedtime. It is well to conjoin with it some drug which will favour the contraction of the weakened cerebral vessels; for this purpose we may give tincture of ergot or tincture of digitalis, one or both. In many cases of chronic wakefulness, arising from mental strain, the patient is distinctly anæmic. Unless the anæmia be remedied the insomnia cannot be cured. The patient's pale face and compressible pulse declare the condition of his blood. Such a person mostly feels drowsy when he is up, and wakeful when he lies down. Of course he needs iron; we may give him a grain or two of reduced iron, sprinkled on a piece of bread, or a wineglassful of Orezza water, after each meal. His diet must be liberal, containing plenty of fish, meat, and eggs. For such a patient alcohol is often the best hypnotic. To many people a "nightcap" of toddy is a superfluous and hurtful luxury. But it can give, perhaps better than anything else, rest and sleep to the exsanguine and worried brain. We must never be blind to the responsibility we incur when we prescribe alcohol. When we use it as a remedy in the treatment of disease we must state distinctly the reasons for its adoption, and we must discontinue it, as we discontinue the employment of other drugs, when the conditions which called for its exhibition have disappeared. If I am sure of anything in therapeutics, I am sure that alcohol is the best hypnotic in many cases of chronic psychic insomnia when the patient is worried, sorrowful, weakly, and anæmic.

Many minor points are worthy of attention in the cure of chronic psychic insomnia. In most cases, whether he sleep badly or well, the patient ought, from day to day, to go to bed and get up at some fixed hour. Healthy sleep tends to occur periodically. Daily bodily exercise, short of great fatigue, must be enjoined. Riding in a carriage is good, walking better, riding on horseback the best of all. A worn and self-worrying man, wrapt up in the absorbing current of self-consciousness, may take exercise in a carriage or upon his legs and still keep up his fretting, but he must come out of himself when he gets into a saddle. Gardening, for those who live in the country, affords good exercise; and is very efficient in keeping up objective attention. Those who live in towns may find good objective employment in chopping wood; if they have not trees to fell, they can at least copy Archbishop Whately, and give their minds a refreshing objective turn and their muscles healthy

work by cutting up firewood. People who find it difficult to get off to sleep are sometimes advised to diligently and monotonously count one, two, three, up to a thousand or more, until they fall asleep; to watch in imagination each one of a large flock of sheep squeezing through a narrow opening; to picture some familiar landscape and keep the mind fixed upon it; to repeat the letters of the alphabet, &c. These are expedients for changing the current of cerebration. For a night or two one or the other may succeed, but they cannot be relied upon. These practices often even keep up wakefulness; when the mind attends closely to them they perpetuate the subjectivity which keeps the brain from resting. Very often the surest way of keeping awake is to try hard to get to sleep. We do most things best when we forget ourselves: going to sleep is no exception to the rule. When the contractility of the cerebral arteries has become much weakened by prolonged thought, and when, as a consequence, there is wakefulness, sleep may often be induced by the temporary application of cold to the general surface of the body. A person who has been lying awake will often fall asleep at once after getting out of bed and sousing his head, neck, and hands in cold water, or after (following Charles Dickens's plan) standing at the bedside until he feels chilly, and turning over, shaking up, and cooling his pillows and the bedclothes.

Just a word about the treatment of the other varieties of insomnia. In the toxic kinds we take away or diminish the tobacco, the alcohol, the tea, &c., as the case may be. *Cessante causâ, cessat et effectus.* A discussion of the treatment of gouty insomnia and of the sleeplessness arising in some chronic renal diseases would involve a consideration of the whole question of the therapeutics of the maladies upon which these forms of wakefulness depend. I shall only say that in gouty lithiasis, with a pulse of high tension, I have confidence in the curative effects of colchicum, supplemented by the exhibition of dilute saline purgatives, such as Pullna, Friedrichshall, Hunyadi Janos, or Rakoczy waters. Senile insomnia is very obstinate; perhaps in the bromides, with full doses of hop or henbane, we have the best and least harmful means for its relief.—*Lancet, June* 22, 1878, *p.* 889.

DISEASES OF THE ORGANS OF RESPIRATION.

19.—THE TREATMENT OF EARLY PHTHISIS.

By Dr. J. MILNER FOTHERGILL, Assistant Physician to the Victoria Park Hospital for Diseases of the Chest.

The leading characteristics of early phthisis are cough, emaciation, loss of flesh, night-sweats, and pyrexia, with more or

less hæmoptysis; each symptom indicating an appropriate line of treatment. For here it is essential to treat symptoms while doing our best to influence favorably the pathological process on which they causally depend. If asked the question, "What do you think the most important matter to attend to in the treatment of early phthisis?" my answer would be "To arrest the night-sweats." "The next most important?" "To keep the stomach and intestines in good order and attend to the assimilative processes." If these are not attended to all treatment is futile, or nearly so. If the sweats are not checked the blood-salts drain out as fast as supplied; if the digestive powers are not cared for, the food taken is not assimilated, and so the patient is no nearer more perfect nutrition and effective tissue repair.

To arrest night-sweats we must have recourse to some anhidrotic, as oxide of zinc, sulphate of copper, or one of the solanaceæ, as hyoscyamus, and still more, belladonna. The first two act as astringents, generally affecting any part where there is an abnormally excessive flux; how, we do not know. Belladonna acts directly upon the secreting nerves of the sudoriparous glands, whether applied locally, or administered by the mouth. Probably hyoscyamus acts in an allied manner. Taken altogether there is no anhidrotic to be compared with belladonna: though in the few cases where it fails the other agents may be tried. But in order to get out the good effects of belladonna, it is necessary to give it in sufficient dose. The ordinary dose of sulphate of atropia—for it is much better to use a solution of atropia of known strength than to give the tincture of belladonna, which may, and probably usually does, vary in strength—is in many cases quite insufficient. The variations of toleration of belladonna in individuals is as pronounced as is the case with Epsom salts; what is sufficient of the latter for one, exercises no influence over another person, while the dose some require to produce even a gentle action of the bowels would produce well-marked, nay, serious diarrhœa in others. I use atropia in doses varying from the seventy-fifth (75th) to the fiftieth (50th), and up to the twenty-fifth (25th) of a grain. A considerable proportion of patients are unaffected until the last dose is reached; and even then do not complain of much dryness of throat, or indistinctness of vision (effect upon the pupil as a guide to the administration of belladonna is utterly worthless). With many patients the seventy-fifth of a grain of atropia will arrest the night-sweats, and in a certain number will affect the throat and eyesight; while others require the fiftieth to influence the night-sweats: and again a small proportion are uninfluenced till the twenty-fifth is reached. Thus we see the toleration of belladonna varies very much with

different individuals. An impression exists in my mind that these large doses of belladonna are more frequently required in the case of Jews than of other patients. The practitioner then must not go away with the impression that belladonna had failed in any case until he has pushed the dose to decided dryness of the throat and distinct impairment of vision; flinging aside any effect upon the pupil as a fallacious test not to be trusted; for in my experience the pupil is rarely much affected; and yet in other cases a marked effect is occasionally produced on the pupil by placing a small belladonna plaster over the heart. To some other effects of belladonna reference will be made shortly.

The profuse night-sweats of phthisis, and at times of other maladies, are very exhausting. Sweat is a secretion which contains chlorides, phosphates, and sulphates of the alkalies, as well as urea, uric acid, traces of iron, and of fat or of fatty acid. Consequently, when the sweat is profuse in a person who is debilitated, it drains the body of its salts, and in doing so cripples the assimilative powers. Usually the first consequence of arresting the night-sweats of the phthisical is the return of the appetite—food is both relished and digested. So long as this drain goes on it is practically useless to give milk, phosphites, meat juice, &c. &c.—it is like pouring them through a sieve. The importance of checking the night-sweats cannot be overrated.

A few words as to the associations of night-sweats may not be out of place or without instructive value. It is well known that ordinarily the night-sweat comes on towards morning—in the deep morning sleep. Often, if the patient keeps awake the sweats do not come on. On the other hand, where deep sleep is produced by an opiate given to relieve the cough, profuse night-sweats are commonly the consequence. These associations of night-sweats are significant. They largely depend upon the relations which exist betwixt the pulmonary and the cutaneous respiration;—relations much more pronounced in human beings than is commonly supposed. Their relations in some of the lower animals are well known. When the respiratory centre is depressed in deep sleep, and the pulmonary respiration is lowered very distinctly, the sudoriparous glands are thrown into action. When the blood is deficiently aerated, and there is an excess of carbonic acid in it, the sensory nerves of the sudoriparous glands are thrown into action and sweating follows. (Ott and Field, Journal of Physiology, 1878). When then the respiratory centre is exhausted by the efforts required to aerate the blood, where the amount of useful lung is limited, and the respiration drops low in deep sleep, sweating, or cutaneous respiration, is the result. Belladonna is a direct stimulant to the respiratory centre when failing, either from

disease or from a toxic agent, and so is useful in two ways. It arrests the action of the sudoriparous glands on the one hand: and by stimulating the respiratory centre on the other does away with the necessity for hidrosis. Consequently it is well to give atropia with morphia whenever it becomes necessary to give the latter drug to relieve the night cough of phthisis. The antagonistic actions of morphia and belladonna are now sufficiently accurately ascertained to enable us to combine them in an intelligent and practically useful manner. Belladonna does not act so powerfully upon the hemispheres as to interefere much with the action of morphia upon them; while its sedative, or paralysant action upon the ends of the vagi (the sensory nerves) in the lungs renders it a useful adjunct to the morphia in arresting cough—a reflex action exerted by the presence of an irritant in the lungs in the form of the neoplastic growth. Not only that, but morphia lowers the activity of the respiratory centres, indeed kills by arresting the respiration, and after it the circulation: while belladonna is a direct stimulant to both. Consequently, even if there be no night-sweats, when it becomes necessary to exhibit opium or morphia for the night-cough of the phthisical it is well to combine with it a dose of atropine, to antagonise the effects upon these rhythmically discharging centres of the respiration and circulation—effects which are unsought and undesirable, yet unavoidable. (For the evidence for these statements the writer must refer the reader to his Essay on the Antagonism of Therapeutic Agents: and what it Teaches, 1878.) If there are already night-sweats the atropia will prevent the opiate making them worse; and often will be found effectual in checking them while not interfering with the desired effects of the opiate. The pill in common use by the writer at Victoria Park Hospital consists of one-fourth of a grain of morphia (hydrochlorate), a fortieth of a grain of atropia, with a grain of capsicum in powder, and three grains of pil aloe et myrrh. At the West London Hospital, of one-third of a grain of morphia with one-thirtieth of a grain of sulphate of atropia. This pill is well borne in almost all cases. The morphia checks the cough and procures sleep, while the aloetic vehicle prevents the bowels being locked up, and the appetite diminished by the action of the opium upon the local ganglia of the intestinal tube, and on the sensory nerves of the stomach. By such a combination indeed we can secure the desired action of the opiate, and get rid of the effects which are objectionable and detrimental to the patient. So far I have never once seen any of the toxic effects of atropia, as dryness of throat and indistinctness of vision, follow the use of this combination; the morphia apparently combating such manifestations. This use of opium and belladonna together will be found most serviceable in practice.

If belladonna pushed freely does not arrest the night-sweats —an occurrence very rarely encountered—then oxide of zinc with hyoscyamus or sulphate of copper with opium, may be tried. Dover's powder, conium, quinine, the mineral acids, or tannin, or gallic acid, or ergot may be tried. Then comes the question of applications to the skin. Vinegar or a weak solution of a mineral acid may be washed over the surface with advantage. Dr. Lewis Sayre informs me that an irregular practitioner in New York many years ago gained a great reputation in the treatment of phthisis by sponging the patient with hot vinegar containing a considerable quantity of powdered capsicum. He was very effective in arresting the night-perspirations; and, as usual, when these exhausting sweats are checked the appetite returns and food is relished and digested. However attained—if attainable at all—the first thing to be done is to check the night-sweats; and the hot vinegar with cayenne pepper is useful in very obstinate cases.

Attention to the stomach and bowels, or, as our predecessors used to say, *primæ viæ*, is essential, and scarcely of secondary importance to the treatment of night-sweats. It may be heterodox to say this in the present worship of physical signs, but it may be said truthfully enough—that with phthisical patients it is more important to study the tongue than to go over the chest with the stethoscope. The latter may doubtless tell the extent of the disease, and so demonstrate the physician's skill in diagnosis: but the other affects the patient; and attention to it may save a life, and neglect of it lose one. When the tongue is covered with a thick fur, it is useless, or nearly so, to give iron and cod-liver oil; for the tongue is the indicator of the state of the intestinal canal, and absorption through the thick layer of dead epithelium cells is well-nigh impossible. It is well here to give a compound calomel and colocynth pill every second night, and to prescribe a mixture of nitro-hydrochloric acid, or phosphoric acid with infusion of cinchona, three times a day, till the tongue cleans. Or at other times the tongue is raw, bare, and denuded of epithelium. Here it is of cardinal importance to put the patient on a mixture of bismuth with an alkali, and a milk dietary. Often milk and seltzer-water will agree where milk alone is too heavy and too constipating. As long as the tongue is raw it is necessary to fight the case on this line; attending to the night-sweats of course, but not attempting to give hæmatics or oil.

Then comes the matter of attention to all drains, such as diarrhœa. The phthisical are readily depressed by diarrhœa, and it should always be attended to energetically. Of course in the later stages, where the intestines are the seat of tuberculous ulceration, the diarrhœa is very intractable, requiring the

free exhibition of bismuth and opium, and even of ipecacuanha, which seems to be of service in such cases. But in the early stages it will yield to a pill of sulphate of copper (half a grain) and extract of opium (one grain). Rice-water as a beverage is indicated where there is a tendency to diarrhœa, and beef-tea should be avoided. Beef-tea often sets up or keeps up a loose action of the bowels. Still more important is it to attend to all drains when the patient is a woman. The neglect of this matter is simply appalling. I have known a woman kept in our most famous hospital for six weeks for a trifling piece of mischief at the tip of one lung, and an attack of hæmoptysis of no great severity, while she was profusely unwell seven out of fourteen days; but it had never struck the physician to inquire into that form of hemorrhage. The woman was drained by menorrhagia and leucorrhœa, but these had never even been asked after. Another patient was some months in the Brompton Hospital for pleural thickening of the left apex, where a similar state of matters existed with ovarian congestion. It is needless to say that in neither case did any improvement result from the stay in hospital. Three years ago, when going over the National Hospital for Consumption at Ventnor, I asked as to how far any systematic inquiry was made into the drains of female patients, and found that no such inquiry was then practised. In ordinary hospitals no arrangements are made, or place provided where women may retire for the purpose of practising vaginal injections or the use of the bidet: to my mind a very reprehensible omission. In many menorrhagic women it is more successful practice to limit the loss of blood at the catamenial period than it is to build up the blood during the intermenstrual interval. As to leucorrhœa, it is a dead loss to the system from every point of view, especially mischievous in the phthisical.

As to diet. It must be nutritious, and easily assimilable. It should consist of meat-juice in any form, milk and farinaceous foods, and especially the different foods prepared for infants, which are mainly starch partially digested. If solid food can be taken well, very good, and a certain amount may be taken daily. Londoners seem to think that mutton is the food for all invalids, from the phthisical to the dyspeptic. Where there is a tendency to diarrhœa it is well to avoid beef-tea, and to resort to a milk dietary. Where the digestive powers are low, meat-juice or raw meat pounded may be digested where starchy foods are not assimilated. But my own opinion is that farinaceous foods are not so objectionable as some would make out, if proper care be taken to see that they are taken as they should be. Thus beef-tea, which alone is scarcely a food, becomes nutritious if biscuit-powder, fine oatmeal, or baked flour under

any name, be added to it. This is better than thickening with isinglass, or gelatine. Then if there be diarrhœa, it is well to make rice-water and use it to dilute the preserved milk, instead of plain hot water. Attention to these trifles may constitute the turning-point of a case. Then milk puddings, stewed fruit and cream, especially where there is any tendency to constipation—or those cakes of oatmeal and treacle sold by Scotch bakers and confectioners, which are a very pleasant laxative food, may be eaten with advantage. It is well that the patient should sleep after the noontide meal; this aids digestion and cuts the weary day in two—no slight matter, especially when the days are long. Then when the digestive powers are feeble, and the patient cannot fast long, it is well to have a glass of milk through the night; or a glass of that excellent old-fashioned remedy, rum and milk, early in the morning; this breaks the fast, and often procures the patient some refreshing sleep ere getting-up time comes. With many, the glass of rum and milk enables them to relish the breakfast when it arrives, where otherwise the long fast would do away with all appetite. The breakfast should consist of coffee, or cocoa, with some good milk, an egg, or a little bacon; and the bread should be cut thin, and the butter rubbed well in. It is well to finish the breakfast with fruit, an omission in English practice that should not exist. A glass of milk, or a biscuit betwixt breakfast and lunch or early dinner, is indicated in some cases, where the patient cannot go long without food; but the too common practice of having a glass of wine at eleven o'clock has no vindication in most cases. Alcohol may be taken with food to aid the digestion, and a glass of sound wine or good malt liquor, at lunch and at supper, is often of service; but the constant sipping of alcohol is bad, and the port-wine treatment of phthisis is unjustifiable, where it is not a hollow mockery and the wine a vile adulteration. A glass of really good port wine at meals suits some invalids better than any other sort of alcohol. Alcohol should be taken as an adjunct to other food—not as a substitute for it. Of course, in the final stages alcohol is sometimes the only food that the patient can take; but it is a well-known fact they do not live long on it.

Such are the lines to be pursued in the treatment of early phthisis. Some intercurrent matters and side issues may now be briefly considered. The first is cough. Where a patient is one of several in a hospital ward it may be necessary for the sake of the others to give the patient a quiet night, as well as desirable for him, or herself. But opiates have drawbacks, and should be combined with other agents, as stated in the commencement of this article. The question of the use of an opiate linctus, "to be taken when the cough is troublesome," is one on

which opinions may differ. My own opinion is dead against it: I have seen the most disastrous consequences follow—loss of appetite, constipation, further loss of flesh, &c. To my patients the advice given is—that it is better to put up with cough; that the "something for the cough" will do more harm than good, and that they are better without it. Some take the advice; others transfer their professional confidence to some physician who holds a different opinion about "cough-medicines": anyhow I do not see much of the slow poisoning (often not so very slow) of phthisical patients by opiate linctus now, having seen quite enough of it. Then there are those abominations called "cough lozenges," which are just as bad as the linctus. I do not dogmatically assert that these things never do good; but the harm done to most cases far counterbalances the good done to the few. If a medical man is called in to see a perfect stranger suffering from a racking cough, he is probably justified in prescribing a sedative cough mixture at first to give relief, and so gain the patient's confidence; but the systematic use of such medicine is too frequently immoral and unjustifiable. As to the use of "cough lozenges" Dr. Mitchell Bruce's view is a sound one; he gives the morphia and ipecacuan lozenge, finding from experience that the ipecacuan generally nauseates the patient before enough of morphia has been taken to do much harm. Where the cough is very troublesome, bromide of potassium may be given as affecting reflex action favourably with a minimum of bad after-effects. The most pleasant means of relieving cough, that is useless and harassing, is hydrobromic acid, with spirits of chloroform three or four times a day; it is effective as well as palatable. Chloral is not a drug to be advocated in cough.—*Practitioner, Sept. and Oct.*, 1878, *pp.* 184, 241.

20.—ON THE TREATMENT OF PHTHISIS AT DAVOS AM PLATZ.

By Dr. T. Clifford Allbutt, Leeds.

For many years I have held that the majority of phthisical patients die of septicæmia, and that the arrest of this daily re-poisoning is a primary object of treatment. To reach, cleanse, and dress ulcers of the lungs by surgical methods seems impossible, and the effects of antiseptic inhalations are disappointing; if, however, there be an antiseptic climate, we may hope to counteract this secondary blood-poisoning by sending our patients to live in it. Such antiseptic or aseptic climates are found in Switzerland, in Upper Egypt, and in other districts; and in the mountain air of Switzerland there is found also a tonic virtue which no doubt largely aids the physician in his work. The aseptic state of the air on alpine heights has been

proved by Professor Tyndall's experiments at the Bel Alp; but the Bel Alp, in common with most other Alpine resorts, is insufficiently sheltered. In Davos alone, so far as we yet know, is found an air at once aseptic, bracing, and still. Upon these points I must refer the reader to my former paper. Very few English invalids have as yet found their way to Davos, and observations based upon the cases of English invalids are therefore not many. During the past winter my friend Dr. Rüdi has treated nineteen cases among the English; some of these were patients of my own, and others had been seen by me. Dr. Rüdi has kindly sent me notes of all. Of these nineteen, nine were cases of pulmonary disease. Shortly put, they fared as follows:—

Case 1.—Arrived on August 6th, 1877. Large cavity in upper lobe of left lung below the clavicle, infiltration of the whole lobe, and moist râles all over the left lung. Ordered to keep quiet, sit out in the fresh air, take abundant food and milk, and wine twice daily. Soon strength was restored, perspiration ceased, fever ceased, and appetite returned. At the end of December came on an attack of bronchitis in the whole left lung and renewed fever; this attack lasted four weeks. Recovery was satisfactory; respiration in the lower lobe became normal, and the cavity almost ceased to secrete. The surrounding tissue is still infiltrated, but shows no sign of irritation. The cavity is contracting. Next winter it will close. Although this patient has to return to Davos good progress was made; for on arrival the disease was still progressing and liquefaction rapidly at work.

Case 2.—Came with infiltration of the right apex and adhesions of the right lung to the diaphragm after pleuro-pneumonia. Perfect recovery in five months.

Case 3.—Had a very large cavity in the right lung, extending into both upper lobes; moist râles all over the right lung, infiltration being extensive. In four months expectoration diminished to one-third, and strength and weight were gained. There was no inherited disposition. The process being now arrested and the cavity closing, recovery may be expected next winter.

Case 4.—Convalescent from a pneumonia. No infiltration. Very feeble breathing, and imperfect expansion of left lobe. Douches used. Recovery perfect.

Case 5.—Course exceptional. The appetite improved directly on arrival, sleep returned, fever and night-sweats ceased, and there was no diarrhœa. The state of the lungs also much improved. But this patient lost constantly in weight. He is a medical man, and says of himself, "in every other respect I made a splendid cure, only the constant loss of weight frightens me." [I may add that this gentleman saw me late in the year, and I

would have dissuaded him from going so late to Davos; but he, knowing too well the probable course of his malady, stopped me, saying quietly he had determined to go. Both lungs were diseased, but I have not the exact notes at hand.—T. C. A.]

Case 6 is one in which I (T. C. A.) am deeply interested. I shall give hereafter some extracts from letters this gentleman has written to me. I urged him decidedly to spend the winter at Davos rather than proceed to Egypt as he had intended. He had cavities at one apex and infiltration at the other. He had been a *poitrinaire* for four years, and when he reached Chur could scarcely walk up to his bedroom. His hectic was continuous and severe. Dr. Rüdi now reports of him:—"After staying here another winter he may return to England, although the infiltration at the back part of the left apex will not have fully disappeared." I shall, as I have said, speak further of Case 6.

The two following cases died.

Case 7.—Had come to me (T. C. A.) for many months in 1876-7, in hopeless phthisis. Both lungs were extensively involved, and he had been repeatedly in bed with intercurrent pneumonia. There were other reasons for despairing of his state, among them great anxiety and pressure of business. As his life was very valuable I named to him last October the chance Davos might give him, and I put fairly before him the risks and the hopes. He decided to go on his own responsibility, and I described his going to his friends as a bold stroke for life. The patient wrote to me several times in good spirits, and assured me how much better he was. His appetite and his strength improved; his fever diminished, but *never ceased*. It was normal some days, but it would then run up in an evening to 100° to 101° F., and Dr. Rüdi always found the physical signs gaining ground rather than giving way, and wrote to me without holding out any hope. He died in the middle of February, but his departure from England is not wholly to be regretted, as he certainly suffered less than he had done at home.

Case 8.—Came to Davos on Dec. 20th, 1877, with both lungs infiltrated, the right more extensively. In right middle lobe a cavity. Fever considerable, 102° to 104°, commonly 103.5°. This never diminished. Death occurred on March 20th following. It was a lost case when patient arrived, and should have been sent much earlier in the year if sent at all.

Case 9.—Arrived Aug. 6th, 1877, very ill. Insufficiency of mitral valves after rheumatic fever; there was also a catarrhal state of the right apex. This was therefore a complicated case. Patient gained weight, recovered well, and was sent home cured as regards his lung.

The rest of the cases contain no phthisis, and are as follows:—

Case 10.—When at Davos I thought the patients had, for the most part, an air of well-being, but Mr. —, on the contrary, looked wretchedly weak, pallid, and ill. Mr. —, subject to catarrh of the stomach, came to Davos Aug. 25th, 1877, aged twenty-five, and left again on April 3rd, 1878. The patient writes as follows:—For years he had suffered from chronic dyspepsia, debility, constipation, headache, &c. In March 1876, was put under a "vegetarian-starvation" diet. In three months lost three stones and a half in weight, and could not rise from bed. Rallied under different treatment, but never regained power of digesting food without great pain and distress. Thus from July, 1876, to June, 1877. Then went to Aix-la-Chapelle, and for six weeks had stomach washed out every morning before breakfast by the stomach syphon. Great relief followed at first, then fell back in all respects, and was very ill. On arrival at Davos had great pain and discomfort after food, gnawing pains in the stomach and arms, &c.; appetite capricious; cold liquids "cut the stomach like a knife;" obstinate constipation; unable to move for three hours after a meal; quite unable to read or write, or even to listen to reading. By November was able to walk five hours a day; could take much more food, and with less discomfort. By April, altogether much stronger, able to read and write, and take plenty of exercise. Could now drink cold liquids at meals, and was in most respects wonderfully better. Digestion remained, however, weak, and diet had still to be restricted to plain meat and bread, and a little fruit. Weight: Aug. 21st, 1877, 7 st. 2 lb.; Sept. 21st, 7 st. 9 lb.; Oct. 19th, 7 st. 12 lb.; Nov. 26th, 8 st. 2 lb.; Dec. 25th, 8 st. 6 lb.; Jan. 22nd, 1878, 8 st. 11 lb.; March 1st, 9 st. 6 lb.

Case 11.—Arrived Nov. 15th, 1877. Hyperæmia of liver; nervous excitement, nocturnal emissions, headache, and debility. Ordered sal. therm. Carol., iron with nux vomica, cold rubbings, and douches. Left on April 10th, a great deal stronger, but not quite recovered.

Case 12.—Arrived on Sept. 14th. Old catarrh of rectum, piles, and recurrent dyspepsia. Ordered cold douche, exercise, and diet. Recovered, and left April 5th, 1878.

Case 13.—Hysteria, neuralgia, slight anteflexio uteri. Left much better than she came, but scarcely fully recovered.

Case 14.—Catarrh of the bowel. This case Dr. Rüdi regards as really incurable, and says she is no better. Patient, however, thinks herself wonderfully stronger, and has been able to enjoy life and to take exercise in a measure unknown to her before her arrival. [T. C. A.]

Case 15.—Chronic dyspepsia and nervous debility and depression. [Sent by me at his own suggestion.—T. C. A.] He left Davos in a very good state of health, and his depression probably will not return. This gentleman had travelled to many health resorts without relief.

Case 16.—Hysteria, anæmia, and neuralgia in a young girl. Ordered milk, exercise, douches. Gained weight considerably. Sent home in five months, cured.

Case 17.—Nervous headache and nocturnal emissions, produced by mental work. Ordered exercise, douche, and nux vomica. Perfect recovery.

Case 18.—Hepatitis chronica. Ordered Tarasp water and exercise. In two months left, much relieved, not quite recovered.

Case 19.—Melancholia supervening on nocturnal emissions and mental strain. Too nervous to stand douches, and was sent to Baden to take them. He is there under treatment.

I have thought it desirable to include all the English cases under treatment, though of course many of these miscellaneous cases are of little value. As a rule, perhaps, nervous diseases would do as well or better elsewhere, but they may serve to illustrate the tonic effects of the climate and the ease with which a winter is tolerated at a height of 5000 feet.

In accordance with my purpose, I now give some extracts from the letters received from the patient indicated as Case 6. I may be permitted to state that his evidence is of the highest possible value. In a letter dated Dec. 31st, 1877, Mr. X. says: "I have gone on, so Rüdi says, steadily improving, and he told me last Saturday that the cavity was beginning to close. There is certainly a depression in the surface above the place where it is supposed to be. I can walk a matter of from six to eight miles in favourable states of the atmosphere— *i. e.* when it is at the coldest and stillest,—doing this work in five hours. I have acquired the habit of walking very slowly. I have had several little colds, which have all passed off without much trouble. In the course of one catarrh I began to spit blood-streaks after exercise, but a few days' resting removed this symptom. My temperature hardly ever rises above the normal, and then I do not think it has touched 100° more than once.... I sleep fairly well, and have a really sound appetite, though I find the digestion rather difficult to keep in order. This is probably due to monotony of diet and want of change in the case of one who, like myself, has never spent three months in the same place since he was thirteen. My spirits are excellent, in spite of the unutterable *ennui* of the promenades and want of society. In this respect Davos seems to me to beat all health places I ever tried. They have always depressed me after a sojourn of a few

weeks. Davos agrees capitally with my wife and children and all our party." In a letter dated "Feb. 6th, 1878, thermometer in sun 141° F., overpoweringly blazing," Mr. X. says: "You are welcome to use any parts of my letters if they can be of the least service to you." On Case 7 he comments as follows: "Poor—— has gone steadily down-hill ever since he came here. One thing I feel inclined to remark, *à propos* of his case, is that he came too late in the season. It was quite cold when he arrived, and he never gained strength enough to be able to take exercise. Exercise is, I am sure, the very soul of the winter cure. A really debilitated patient ought to come here in the early half of August, and if he cannot get up his strength by the middle of November, to leave before the snowfall.... I am sure that really bad invalids coming here should be advised, at any cost, to get south-rooms and a large room. Many people seem to me to suffer from crowded sitting-rooms and from sleeping in north rooms. Unless they approximate, at any rate, to the ideal in this respect, I have a strong instinct they will not give Davos a fair chance. We have had a large proportion of days this winter when even an invalid so comparatively active as I am had to keep the house. Then the question of rooms is serious." In some interesting passages in another letter, Mr. X. wonders whether the very rest to the emotions obtained by residence in "quiet and non-human circumstances" does not help to work out a cure, and whether emotional impressions do not tend to have a directly injurious effect upon a diseased organ.

Another able correspondent writes to me on Feb. 9th, 1878: "No doubt you think there must be a great deal of time for letter-writing here, but I assure you it is not so; the days are not half long enough for all one wants to do; then, of course, out-door exercise and amusements take up a good deal of the day, as one feels that to be in the open air as much as possible forms much of the whole duty of man. It is said by regular Davosers that this has been an exceptionally bad winter. We have had so much snow and bad snow-storms, lasting three or four days. Yet the days on which we have not been able to go out at all have been few and far between. Even my invalid sister (Case 14) has often taken her constitutional on the boarded terrace while it has been snowing fast; and we have had the most glorious days to repay us for any amount of snow. Since this month began it has been splendidly fine, with the exception of a little snow on one morning. Consequently we have been skating, sledging, and expeditioning as much as possible. You may imagine how much better my sister is, or she could not be equal to such excursions. I am thankful to say she is decidedly better. Though she still suffers from pain in her head and has not much appetite, yet in essential points there is a great improvement:

the complaint is better, she takes little medicine, she is stronger, and walks and sleeps better." Next comes an account of indoor amusements—"glee-singing, tableaux vivants, German cotillons, charades, a play and Shakspeare readings once a week, with a concert now and then." Of Mr.—(Case 10) the letter says: "You would be surprised at the change in him; he looks, and is, wonderfully better, and takes part in all the entertainments in the most vigorous manner. ....., Mr. X. also [my former correspondent, T. C. A.] looks quite a different man to what he did. Mrs. X. was telling us to-day how much and how steadily he was improving in health. Most of the invalids in this house are making good progress, except poor Mr. —(Case 7), who is very ill, and an Italian lady, whom the doctor has sent away."

These letters give some pleasant insight into the winter life at Davos; and the remarks of Mr. X. on the evil of stuffy rooms are most important when considered in the light of the aseptic hypothesis. Not only so, but I have ventured to urge upon the Kurverein in the strongest manner the duty of securing pure air both within and without the houses. I believe the Verein is in some measure alive to this; scarcely sufficiently so; yet it is a vital point. Let the district once be saturated with sewage, and the virtues of Davos may vanish. Of the manuring of the meadows, I am assured that this is almost all done at snow-melting, when no patients are in Davos. Then the winter's dung is put out. The October dressing is a limited affair, as the cows have lived all summer in the Alps, and there are no great accumulations. During the snow-melting the visitors depart to Italy, or to Baden-Baden.

To those who are thinking of passing the summer at Davos, I would advise that they leave England as the summer begins, travel slowly through the continent, stop a few nights at Ragatz, and arrive at Davos about June 20th.—*Lancet, June* 8, 1878, *p.* 824.

21.—ON ASTHMA.

By Dr. J. B. BERKART, Assistant-Physician to the City of London Hospital for Diseases of the Chest.

Like most other morbid phenomena, asthma also depends upon the concurrence of *predisposing and exciting causes.* Of these etiological elements, the former are decidedly the more important. They alone are capable of explaining why, of two individuals who exposed themselves to the same injurious influence, the one escapes unhurt, whereas the other is seized with dyspnœa. The present inquiry has shown that the tendency to asthma is produced by definite organic lesions of the respiratory

organs. Only the imperfect means of diagnosis and the rarely fatal issue of the disease, at the time when its clinical features are most characteristic, are the cause that the existence of those lesions has been so constantly overlooked. Still, the known pathology of the antecedents and sequelæ permit the safe inference that the stage intermediate between them also corresponds to certain textural changes.

1. *Anatomical Lesions.* — The anatomical details vary in extent and intensity. A copious exudation of serum and white blood corpuscles is already stated to be the starting-point of the affection. Constitutional and local causes determine the subsequent fate of that exudation. Of the former, little is definitely known; only this much may be supposed, that the blood must be deprived of some of its essential properties on account of the great loss of its constituents, which, after their complete development, are destined to perform the most important functions, but whose regeneration is retarded owing to the existing febrile process. As regards the local causes, there is reason to believe that the crowding of the leucocytes in the bronchial and interstitial tissue not only impedes their development, but leads to compression and obliteration of the adjoining capillaries. An extensive thrombosis of the capillaries, whether from stasis, compression, or endarteritis, thus diminishes the supply of arterial blood to the affected parts. The white blood-corpuscles are softened and reabsorbed. This process of liquefaction implicates, at the same time, the tissues in which it takes place. The bronchial muscles, paralysed by the preceding inflammation, now undergo fatty degeneration; while the elastic fibres become hyaline and brittle, each fragment being composed of round or oval granules. Atrophy of the contractile and elastic element of the lung is, then, the result of those degenerative changes. As the pleural tension, however, remains the same, while the resistance of the atrophic lungs to the excentric traction is diminished, the bronchioles and alveoli are consequently dilated. This atrophous dilatation is either cylindrical or saccular, and is most marked in the bronchioles of the fourth subdivision, extending hence to the alveoli, so that the dilatation of the one occasionally preponderates over the other. Such lungs, inflated and dried, present the aspect of a large-holed sponge, the cavities varying in size from that of a hemp-seed to that of a pullet's egg.

Not unfrequently the white blood-corpuscles are, for various reasons, endowed with sufficient vitality, so as to develop themselves into permanent tissue. An active hyperplasia and hypertrophy are then observable in the parenchymatous stroma of the lungs. This genuine peribronchitis and interstitial pneumonia also lead to obliteration of capillaries and air-

vesicles by the firm connective tissue, the fibres of which radiate in all directions; while the contractile and secreting structures of the bronchi are either destroyed or greatly impaired in their functions and nutrition. Here, again, the tonus and elasticity of the lungs are so greatly diminished, that the pleural tension readily effects the "hypertrophous bronchiectasis" and the vicarious emphysema.

Unless the volume of the blood decreases in proportion to the reduction of the respiratory surface, it is evident that the pervious vessels of the lungs must be in a state of vicarious distension. But the destruction of the pulmonary capillaries generally takes place at the time when development is most active, and when, with the growth of the osseous system more particularly, the volume of the blood also increases. Hence, the greater amount of work which the right side of the heart is now called upon to perform, in order to force a relatively larger quantity of blood through the contracted lungs, produces hypertrophy of the organ,—a consequence to which Dr. Peacock was the first to call attention. Under the influence of this increased pressure, the pulmonary vessels, unprovided as they are with a vascular tonus, readily dilate; and, in order to compensate for the loss of the capillaries, extensive anastomoses form between the pulmonary artery, on the one hand, and the bronchial and pulmonary veins on the other, by means of vascular arches, remarkable on account of their great length, their equable diameter, and their want of branches. The surface of the bronchial mucous membrane then appears studded with granular or villous elevations; on microscopic examination, these are found to be vascular papillæ, pyramidal or club-shaped, consisting of a tortuous vessel, elevated over the surface and surrounded with connective tissue.

The inundation of the pulmonary parenchyma with nutritive fluid causes enlargement of the bronchial glands, which, as a rule, participate in all affections of either the surface or substance of the lungs. In many instances, that enlargement dates from the whooping-cough and measles, and diminishes or increases with the diminution or increase of the quantity of, as well as the obstacles to, the lymphatic current. Other changes in the bronchial glands, beyond the mere hypertrophy, are mainly determined by the constitutional proclivities of the patients.

So long as the energy of the heart remains unimpaired, the organism is in no perceptible manner affected by the reduction of the respiratory surface, but when the cardiac muscle degenerates, as it necessarily does, in consequence of the overstrain, a general dilatation of the venous system manifests itself. Varicose veins of the lower extremities, hemorrhoids, varicocele, and

dilatation of the ovarian and uterine plexus, are almost constant occurrences in asthma. Less frequent, but by no means rare, is the enlargement of the thyroid gland, which apparently comports itself towards the lungs as the spleen does towards the liver in cases of hepatic cirrhosis. Occasionally, under the influence of the increased venous pressure, a kind of erectile tissue forms upon the turbinated bones, and naso-pharyngeal polypi develop themselves, which Voltolini erroneously regards as causes of asthma.

According to the observations of Verneuil and the experiments of Ranvier, the varicose dilatation of a vein always commences at its radicles. As the healthy nutrition and function of an organ depend, to a great extent, upon the integrity of its capillaries, it is readily conceivable that profound changes of the surrounding tissues are liable to occur and actually take place in consequence of that dilatation; for the increased pressure in, and the thinning of, the venous radicles, favour the escape of serum and white blood-corpuscles. The various "catarrhal" affections of the external and internal linings of the body,—the skin and mucous membranes,—are thus the consequence of, and in their turn increase the tendency to, asthma.

2. *Diathetic Disorders.*—The connection between skin-diseases and gout on the one hand, and asthma on the other, has been somewhat exaggerated and also perverted. The theory of Duclos and Trousseau, according to which a "herpetic principle" in the blood attacks in a fitful manner, at one time the skin, at another the bronchial mucous membrane, is devoid of all positive proof. All the reported cases, upon which that theory relies, are certainly not conclusive in its favour. A house-painter suffers in 1859 from eczema, which completely subsides; but in 1861 he has a dyspnœal seizure. Moutard Martin observed a case in which an eczematous eruption was followed, at an interval of *six years*, by "asthma." What influence the cutaneous affection could have had in these cases upon the appearance of the dyspnœal seizures, is beyond comprehension. If, nevertheless, skin-diseases and asthma are frequently associated, the reason of that occurrence must be sought in the nutritive changes, which, by the dilated venous radicles are produced in the external integument. "Eczema" of the lower extremities generally accompanies the varicose dilatation of their veins; but this vesicular eruption is only an indication of the facility with which serum and white blood-corpuscles escape through the altered vascular walls. Under these circumstances, the causes productive of cutaneous affections find a very favourable soil for the display of their effect in the asthmatic. And, as extensive congestion of the skin would necessarily pro-

duce a collateral anæmia of the lungs, in the same manner as, but much more effectually than, a sinapism,—judging from the extent of the surface implicated, — the alternation between asthma and a dermatosis can be readily accounted for.

A similar interpretation may, perhaps, be placed upon the association of gout and asthma. Both seem to be the co-ordinate results of the same constitutional cause; for an absolute or relative plethora, such as it undoubtedly exists in both diseases, is sufficient to explain the origin of the organic lesions of the lungs, as well as that of the articular affections. As regards the supposed retrocession of gout to the lungs, there is, as yet, no fully ascertained case in which its characteristic deposit has been found in that organ. But if lithic acid were to accumulate in the lungs, there is little doubt that it would here produce the same effect as it does in the joints.

3. *Inheritance.*—A hereditary element seems to be influential in the production of the asthmatic tendency. About forty per cent. of the patients state that some members of their families had been subject to the disease. Yet in eleven out of seventeen cases, that hereditary predisposition is traceable to grandfathers and grandmothers. Considering, however, the vague application of the term asthma, and the fact that atrophic emphysema, with its intermittent dyspnœa, is almost a physiological attribute of old age, no great reliance can be placed upon those statements. This much, however, is certain, that feeble parents produce a feeble offspring, and that just as physiognomy and mental disposition are transmitted, so a peculiar weakness of texture and a deficient power of resistance to, and of repair after, injury, may be communicated from one to another generation. Where the paroxysms appear soon after birth, it does not follow that, as Hyde Salter suggests, the disease is congenital. A premature respiration of the fœtus on its passage through the maternal canal may, by the aspiration of fluid, lead to atelectasis or bronchitis.

4. *Age.*—The first ten years of life are most obnoxious to asthma, and the reason is, that infantile bronchitis and catarrhal pneumonia, complicating measles, and whooping cough, are most frequent at that time. But though the foundation of the disease may be laid at so early a period, its manifestation depends upon the extent and intensity of the primary affection. In comparatively rare instances, the predisposition slowly develops, and declares itself only in youth and early manhood. At a more advanced period of life, the involution of the lungs, premature or in connection with that of the body in general, constitutes a very marked liability to the dyspnœal paroxysms. In old age, however, asthma soon loses its peculiar characteristics, inasmuch as the intermittent is converted into a permanent

dyspnœa, while the anatomical lesions, upon which this depends, are readily discernible.

5. *Sex.*—Asthma is decidedly more prevalent among men than women. How far the wear and tear of the body, proceeding from habits and occupation, are accountable for its greater prevalence in the male sex, is not quite known. The supposition that those suffer most who most expose themselves to the inclemencies of the weather, as, for instance, "costermongers, cabmen, and Covent Garden porters," is not borne out by facts. On the contrary, those who lead a sedentary and indoor life supply the greatest contingent of asthmatics. The occupation, however, can have no bearing upon the question in the first ten years of life, where that preponderance of the male sex is already noticeable. It appears that the greater frequency of asthma in the latter is mainly due to the greater mortality of the female from whooping-cough.

6. *Occupation.*—According to Hyde Salter's statistics, about seven-ninths of the male asthmatics are gentlemen, and more than three-fourths of the female asthmatics are ladies. Thus the upper-middle and upper classes would seem to contribute more than three times as many cases of asthma as the lower. Salter suggests that the greater frequency of the disease among the better classes may be attributable to the promptitude with which they bring their suffering under medical cognisance. Much, however, is due to the medical and sanitary advantages which the children of the rich possess. They are thus enabled to pull through the catarrhal pneumonia that lays the foundation of their disease, and to survive as asthmatics; whereas the children of the poor, when suffering from that affection, are cut off at once. Statistics of those who seek advice at hospitals are not at hand. This much, however, is certain, that chronic bronchitis, with dyspnœal paroxysms. which among the well-to-do is generally called spasmodic asthma, is by no means of rare occurrence among the poor. Still, it appears as if the children of the rich were more liable to catarrhal pneumonia than those of the poor; for, while the latter pass, ill-clad and exposed to the inclemencies of the weather, through their whooping-cough, in the streets, with no other inconvenience than the temporary interruption of their play by a fit of coughing, the former are confined to rooms, and are anxiously guarded against the access of air—the much-dreaded draught. The observations of Bartels tend to show that in badly ventilated rooms inflammation of the bronchi rapidly spreads to the pulmonary tissue. And Geigel also states that, while the most neglected children suffer mainly from derangement of digestion, those better cared for are chiefly subject to pulmonary affections.

The dyspnœa peculiar to asthma is distinguished by its type. Expiration is relatively more difficult than inspiration. The reason is obvious. Inspiration is the result of muscular activity, so that the lungs yield to the traction which is greater than the resistance they are capable of offering. Expiration, on the contrary, is actively and mainly performed by the lungs themselves, and the want of their co-operation in this act cannot be fully compensated for by the aid of the auxiliary forces. Now, the predisposition to asthma consists of nutritive changes of the smaller tubes and of the air-vesicles. These portions of the organ are truly the *loci minimæ resistentiæ;* they are the parts first and chiefly affected by noxious agents, and are thus readily disabled in the performance of their function. The intensity of the subjective symptoms is in a great measure attributable to the same cause. Even in the healthy, an obstacle to expiration produces *far greater* distress than one to inspiration, or to both phases. But here, the lungs are, on account of their diminished resistance, widely distended, and this distension considerably increases the difficulty of expiration.

The *immediate causes of asthma* are numerous and of various kinds, but all productive of the same results—obstruction of the bronchi. They are exhibited in the following table:—

I. Hyperæmia and œdema of the lungs.
   - *a.* Relaxative { Inhalation of irritating gases; Inhalation of dust; Thermal influence.
   - *b.* Collateral { Meteorismus; Embolism of the pulmonary artery.
   - *c.* Reflex paralytic.

II. Bronchitis.

III. Stenosis of the bronchi { Foreign bodies. Compression.

Hyperœmia and Œdema.—The dilated capillaries, and more especially the vascular papillæ, which, as above described, project from the surface of the mucous membrane, are for various reasons greatly susceptible to fluxionary hyperæmia. Such turgescence readily causes occlusion of the bronchioles. The inspiratory traction, however, is soon increased in force, so as to enable it to effect the entrance of air into the lungs. The dyspnœa that nevertheless accompanies the hyperæmia is, therefore, due not so much to the absence of air as to the rapidity with which the blood passes through the dilated capillaries. There is no time available for the interchange of gases, and the blood thus arrives at the pulmonary veins in almost the same condition as it left the pulmonary artery. A similar process takes place in the parotid, in which, after section of the sympa-

thetic, the blood in the veins presents the arterial character. The arterial system is thus supplied with blood imperfectly oxygenated. Meanwhile air continues to enter the lungs, whereas none, or very little, can escape from them. The thorax is gradually distended to its utmost limits, and is held in the extreme inspiratory position by the tetanic contractions of the respiratory muscles; the stimulus to these contractions being furnished by the carbonic acid in the blood. When the dyspnœa has reached its climax, amelioration generally takes place. It appears that the carbonic acid in the blood causes contraction of the pulmonary capillaries, either by irritation of the vaso-motor nerves or of the vascular walls. The current of the blood once being slackened there is now no obstacle to the ventilation of the lungs.

This form of hyperæmia is generally of short duration, and does not give rise to exudation. But when the left ventricle is incapable of propelling all the blood that by the increased afflux is brought to it, a passive congestion complicates the fluxion. In consequence of the greater pressure, serum now readily transudes into all the interstices of the lungs. Such œdema most frequently occurs in hydræmia, especially of parenchymatous nephritis, and arises also from the temporary increase of the volume of the blood by an excessive accumulation of fluid in the system.

Fluxionary hyperæmia is caused by irritants, which act directly upon the muscles or the ganglionic apparatus of the blood-vessels and causes them to dilate. It arises from—

The inhalation of irritating gases.—Some asthmatics believe that the air of certain places is particularly obnoxious to them, and they attribute this injuriousness to subtle atmospheric conditions. There is no doubt that every open or confined space, every field, and every house in town and country, possess what may be called their own peculiar climate. But nothing justifies the assumption that the *normal* variations of the atmosphere are capable of producing dyspnœa, however delicate the bronchial surface may be. The mystery that surrounds these cases can, however, be readily solved; for when the "inscrutable differences of air" are responsible for the asthmatic seizures, their hurtful influence proceeds from the presence of irritating gases. Hyde Salter reports, "Mr. C. has been asthmatic from his youth, and suffered at times severely; but for the last four years, during which he has lived in the same house, has had no attack. Last week he removed his residence about four or five hundred yards, to a higher spot, but more closely built round, and *very near to the exit of a sewer;* the third night he slept in his new abode he had an attack of asthma, and is suffering from a repeated attack at this moment."

The exciting cause of this case is so evident as to need no mention; yet the conclusion that is drawn from it is that "the change of locality here was only a distance of four or five hundred yards, but it was sufficient to light up the disease that had been in abeyance for years."

A temporary exposure to noxious gases, which, from decaying organic substances, or in the process of the various manufactures, are discharged into the open air, hardly produces an ill effect, because the space is so vast and the movement in it so continuous that those vapours are rapidly diluted, and thus rendered harmless. Dyspnœa, however, results when their ascent is impeded so that a quantity of them remains within the zone from which the inspiratory current is immediately drawn. In this way fog exerts its injurious influence on the diseased mucous membrane of the respiratory passages; for, besides ill-defined hydrocarbons, it contains large quantities of ammonia, of which Boussingault once found the enormous proportion of one grain in thirty-five ounces of the condensed water. Considering that the injection of a few drops of ammonia into the bronchi causes the most intense pneumonia, the irritating effect produced by fog is readily intelligible.

The air of bedrooms, especially in the winter, proves, perhaps, the most frequent cause of the "nocturnal" asthma. Notwithstanding the diminished energy of all the vital changes during sleep, at least one thousand cubic feet of air pass every hour through the lungs, and return from them charged with more than four per cent. of carbonic acid, and completely saturated with water vapour; and the expired air contains ammonia, probably from decayed teeth, or from particles of food decomposing in the mouth, also hydrogen, hydrocarbons, and sulphuretted hydrogen, which in consequence of a faulty digestion may diffuse themselves into the intestinal veins, and be eliminated by the breath. The products of perspiration and those derived from the combustion of candles, lamps, or gas, contribute in their turn to increase the insalubrity of the place. It is known that the air which contains one per mille of carbonic acid is irrespirable, and its injuriousness is due, not so much perhaps to that gas itself—which when pure, may without harm indeed be inhaled in a somewhat larger quantity than is then present—but to the organic substances which always accompany it. Yet even a slight excess of the pure carbonic acid causes turgescence, a sensation of heat and pricking in the conjunctival and respiratory mucous membranes, while at the same time it increases the cutaneous and mucous secretions. In the presence of organic substances, however, a much smaller quantity of it becomes highly irritative. The inflamed mucous membrane of the respiratory tract seems

particularly susceptible to its influence, and Hawke is led to conclude that the coughing fits of pertussis are traceable to that source. The manner in which the organic substances act is as yet obscure. If it were permissible to speculate on the subject, the dyspnœa, which they undoubtedly produce, may perhaps be attributed to the "oxysulphide of carbon," which, according to Radziewsky, arises from putrefying organic substances, and is widely spread in nature. Radziewsky himself, on inhaling oxysulphide of carbon, immediately felt a pressure on the head, a tendency to vertigo, and constriction across the chest; there was, moreover, the sensation as if the air-passages were obstructed. He soon, however, recovered on exposure to the open air. Experiments on animals gave the same results—dyspnœa and great distension of the thorax. Now, however large a bedroom may be, a few hours' occupation of it will render it insalubrious in the manner just mentioned, unless the air be constantly renewed to the extent of at least two thousand cubic feet per hour and person. How imperfect even such ventilation must be, as regards the removal of the gaseous impurities, in comparison to their dilution out of doors, is evident from the fact that here more than six millions cubic feet of air are available for the purpose. Unfortunately asthmatics, as a rule, adopt no measures for the supply of fresh air. They rely on natural ventilation, or persuade themselves, if this matter even receives their attention, that opening the door of the bedroom is all that is wanted. But the consequence of that neglect is that on account of the slow diffusion of gases the patients are surrounded by the noxious exhalations which they themselves produce. As the temperature of the room is raised by respiration, foul gases from kitchen sewers and other sources are thus attracted into the room more readily than fresh air. After several hours, the atmosphere becomes so vitiated that respiration is impossible; so that towards morning the patient is roused from his sleep by an attack of asthma, and he is forced to seek relief at the open window.

Inhalation of Dust.—Foreign bodies, of different kinds and in various quantities, are at all times present in the atmosphere. The dust in the open air is composed chiefly of inorganic particles, and amounts, as for instance in the streets of Paris, to about one-sixth of a grain in every cubic foot. Indoors it mainly consists of organic substances and more especially of living organisms, whose development and multiplication here meet with favourable conditions. So long as those solid particles are undisturbed and are permitted to follow their own gravitation their tendency is to settle on some object, and thus far they are harmless. But when an external moving force returns them into the air, they may be brought within reach of

the inspiratory current, and thus made to enter the air passages. Their effects vary according to their nature and quantity. Fragments of coal, of iron, of stone, and of glass lacerate the mucous membrane; hairs, feathers, fibres of wool and of cotton, once impregnated with moisture, firmly adhere to the secreting surface, and produce great irritation. Infusoria, of which fifty-six species have been observed in the air, are raised by the wind from drying waters and carried great distances; and though apparently dead, they begin, in contact with the mucus, to loosen the covers which enclose their withering bodies and probably regain a new vitality. Algoid cells and fungi are particularly abundant in ill-ventilated rooms; living, and ready to undergo development on meeting with suitable conditions, they rapidly germinate, or are already germinating, when they are deposited in the air-passages; some of them form a mycelium or masses of toruloid cells; others produce their characteristic heads of fructification. One circumstance connected with the life of fungi, deserves here, perhaps, a passing notice. On watching Botrytis cinerea, taken from a rotten apple, Rindfleisch found that the formation of its spores took place only at night. Subsequent observation has fully confirmed this exclusively nocturnal growth. The want of light had not the same effect, for the spores kept in darkness did not proliferate. But whether the organic substances that accumulate during the night in confined places have any influence upon that phenomenon must for the present remain a matter of conjecture: much less is there ground for supposing that the nocturnal appearance of the dyspnœa is connected with the greater vitality of these fungi at the time.

However, the result of such inhalation is invariably the same —fluxionary hyperæmia, and even inflammation, of the affected portion of the mucous membrane. The *colds* which *draughts* produce arise in that way. It is the current of air that lifts the particles of dust from carpets and curtains, from walls and furniture, and brings them in contact with the lining of the respiratory tract. The serious affections of the lungs which develop themselves from a prolonged exposure to foreign bodies in the air, are exemplified by the diseases to which knife-grinders, masons, weavers, and other operatives are especially subject. Little regard, however, is paid to the "invisible nastiness," as Professor Tyndall calls it, which constantly exists in ill-ventilated rooms. The bronchitis so often attributed to "cold" is, in reality, due to the inhalation of fragments of glass and stone which on *post-mortem* examination are found in the air-passages, or to the fibres of wool which are seen in the larynx and appear in the sputa. Organic substances are even more powerful. The influence of Chætomium elatum and

Penicillium glaucum has already been mentioned. The accidental inhalation of a small quantity of flax meal has been known to produce intense bronchitis and dyspnœa. The dust-like spores of a large puff-ball caused in the same way so great irritation of the air-passages that the patient was confined to his room for several days. In isolated instances the appearance of an asthmatic seizure may be directly traced to foreign bodies in the air, such, for instance, as mustard and ipecacuanha powder. In the majority of cases, however, the exciting cause escapes detection. But it is well to remember that asthmatics are particularly susceptible to that kind of irritation. Not only is their respiratory surface always more or less diseased, but the chronic congestion of the nasal mucous membrane, or the presence of a nasal polypus, forces them to breathe through the mouth, and thus deprives them of the important filtering apparatus. When the nose is pervious, foreign bodies do not reach further than the pharynx; but when that canal is obstructed, they readily enter, not only the bronchi, but are found also in the substance of the lungs. Pulverulent bodies penetrate even to the bronchial glands.

The inhalation of pollen gives rise to the peculiar symptoms known as *hay-fever* or hay-asthma. Ever since attention has been directed to them, their origin has been connected with the hay season, but has been erroneously attributed to the influence of heat and light, or to the odour of certain plants. Mr. Wyman persists in this error, and still maintains that hay-asthma is a purely nervous affection. But the researches of Dr. Blackley have conclusively established the dependence of the disease upon the presence of pollen in the air. He found that from the end of May to the end of July the atmosphere of certain localities contains pollen grains in varying quantities. A small portion of that substance, even less than one two-hundredth of a grain in weight, causes, if directly applied to the mucous membrane of the nose, within five minutes, occlusion of the nasal passages with sneezing and lachrymation. The inhalation of a larger portion of it is invariably attended by a sharp attack of asthma. The severity of this abates with the diminished production of the agent, and disappears altogether when this ceases to be generated. This irritating effect is possessed by the pollen of all plants, but more especially by that of the Graminaceæ, or among the cereals by that of rye.

The action of pollen upon the mucous membrane may perhaps be explained by its behaviour in contact with water-vapour, and also with mucus. Under the microscope, pollen appears as a simple cell, with granular contents, and a cell-wall, consisting of an outer and an inner membrane. The extine is coated with an oleo-resin, of a rich amber, or, at times, a pale

straw-colour, and contains several slits or pores, across which the intine is stretched so as to impede the escape of the granular matter. It does not appear that the oleo-resin has any specific action beyond enabling the grains to adhere to objects to which they may approach. On the addition of fluid, the cell is distended; the inner membrane projects through the slits of the outer in the shape of small mastoid processes, to which the granules are attracted. A further distension causes the membrane to burst, and the granular contents are, with a considerable force, ejaculated into the surrounding fluid. These granules irritate the mucous membrane, especially the mucous glands, and reappear finally in the sputa, resembling zoogloea masses.

Dr. Salisbury attributes the cause of hay-asthma to the presence of an animalcular organism, the *asthmatos ciliaris*. This parasite is said to inhabit stagnant as well as running waters, and to develop itself also on fermenting organic matter. It is said to be spherical or oval in shape, and to consist of a simple sac, with one or more large nuclei, and many smaller granules of various sizes. The sac itself is armed with cilia, and, between them, with a long proboscis. The young are developed within the parent cell, and are discharged from the end opposite the cilia. Dr. Salisbury's statements, however, have not yet been confirmed by any other observer.

E. Lewy observed asthmatic seizures as an isolated affection in 26 out of 1,186 cases of saturnismus. He attributes the dyspnœa to the lead dust, which, from the packing of white lead and the cleaning of type cases, escapes into the air. The inhalation of the lead dust is immediately followed by dyspnœa, palpitation, stitches in the diaphragmatic region, and cough. In from six to seven hours the typical attack of asthma is developed. Lead appears to contract the vessels upon which it acts, while those that escape its influence become congested.

2. *Thermal influence.*—There is a widespread belief that "cold," by inducing bronchitis, is a frequent provocative of asthma. Every patient knows instances, where he himself or his friends suffered from dyspnœa, which he could account for only by a loss of temperature. His opinion fully accords with the prevalent doctrine on that point. Clinical observers of the highest repute constantly advert to the danger of a chill, and maintain that, even in well-appointed hospitals, a patient in bed may "catch cold" (Lebert). Indeed, so powerful and so ubiquitous is this agent, that, according to the text books of medicine, it is at the root of almost all diseases, from cerebro-spinal meningitis to diarrhœa; from acute rheumatism and endocarditis to a sneezing fit.

Formerly it was supposed that a chill checked the cutaneous perspiration, and that an excrementitious substance, retained

in the blood, produced inflammation of internal organs. The varnishing of animals, and the serious symptoms that generally follow these experiments, unless proper precautions are taken, were supposed to confirm that opinion. No poisonous substance, however, was found in the blood, and it was soon admitted that the symptoms of varnishing do not correspond to those of "colds." That theory therefore was abandoned. At present the cause of the disease is sought in disturbances of the economy of heat. Two different views are held on the subject. According to the one, a *sudden* change of temperature, however slight it may be, produces one-sided radiation of heat, and leads through the vaso-motor nerves of the skin to circulatory derangements of internal organs. Professor Pettenkofer insists on the *suddenness* with which the loss of temperature is effected. The difference between the rapid and the gradual abstraction of heat may, in his opinion, be compared, so far as the results are concerned, to a man going or falling down stairs. But if one-sided radiation had really such injurious consequences, there could not be a more dangerous instrument than a fan in the hand of a lady. As a matter of fact, it is not so dangerous as alleged; and the reason is that if, adopting the simile, the man were of caoutchouc, or at any rate, provided with the same elasticity as the regulators of temperature are endowed with, it would be perfectly immaterial, so far as the consequences are concerned, in what manner he descends. Rosenthal equally regards the *suddenness* of the changes as the cause of "cold." His experiments led him to conclude that when an overheated animal, in which the cutaneous capillaries are paralysed, is suddenly exposed to a lower temperature, the blood is rapidly cooled, and in that condition it is returned to the internal organs. It then produces inflammation of those parts that are "loci minoris resistentiæ." In man, a *rise* of temperature follows the application of cold to the heated surface, and there is little doubt that the same law holds good also in animals. But the *sudden return* of the blood implies a contraction of the cutaneous vessels; and, as their paralysis is essential to the cooling, there cannot be a great loss of heat; or, if the vessels were paralysed to the extent that they have lost their physical property of contracting upon the application of cold, no blood could be returned from the surface to the centre. Without entering upon the questions that are involved in this subject, it suffices to say that over-heated (*i.e.*, febrile) bodies *do not catch cold*. A reduction of their temperature tends to re-establish the healthy balance. The success of modern anti-febrile treatment is based upon that principle, which a thousand-fold experience has shown to be correct.

According to another view, "colds" arise from an appreciable

loss of temperature, such as is produced by currents of air and copious evaporation from the perspiring skin. Hirsch concludes that "colds" are most frequent when the temperature and the dew-point of the air approximately coincide, as in spring and autumn. The opinion of Hirsch is, at any rate, based on very extensive statistics; and it is deduced from incongruous facts, which allow no other generalisation. Yet meteorological reports, on however vast a scale, are alone insufficient to solve this obscure problem. At the present stage of civilisation, every individual creates for himself, or otherwise obtains, an atmosphere in which he thrives or decays. The state of the weather will undoubtedly affect, in a different manner, the inhabitant of Belgravia and of Bethnal Green. Both may bestow the same anxious and efficient care to ward off the cold; yet, while the one escapes unhurt, the other catches bronchitis. The influence of the weather would here be very remote. Its injuriousness would be admissible only if it could be shown that those who most expose themselves to the supposed cause, are also the most frequent sufferers of its assumed consequences; and herein statistics not only fail, but prove just the opposite.

These considerations are dictated by what daily experience suggests, and they are decidedly adverse to the prevalent doctrine. It is needless to refer to those well-known instances where delicate as well as strong persons daily sustain, deliberately or unconsciously, considerable losses of temperature. Yet in no single instance has such loss *alone* produced inflammation of an internal organ. The question is not whether cold can, under no conditions, be hurtful. Nobody doubts that, if an animal be kept in a tin box surrounded with ice, the temperature of the body will decrease to 18° or 20° C., and that death may thus ensue from paralysis of the heart and œdema of the lungs. These are *experiments* that happily have their counterpart but rarely in practice. But that is not the point at issue. The supposition is that if a few square inches of skin were denuded of their customary covering, and exposed even to the warm temperature of a room, the consequence would be an inflammatory affection of the muscles or of an internal organ. Let the worst that may, happen in such a case. Let the blood in that circumscribed space, and even to a certain depth, exchange its temperature with that of the atmosphere. Let the vessels at that spot be contracted, and the mass of blood in other organs be correspondingly increased. What can follow from all this? The body produces more heat than it can retain, so that the loss is readily compensated. The increase of the vascular pressure would not sensibly influence even that of the muscles of the extremities, where such differences are generally balanced; and since ligation of the abdominal aorta does not

produce hyperæmia of the lungs, it can hardly be expected that the quantity of blood displaced from a few square inches of skin could have that effect. Granted even that a hyperæmia resulted, there is still a wide difference between the temporary turgescence and inflammation. But facts are altogether opposed to that theory. Jürgenssen rapidly reduced the rectal temperature of a man to 33° C.; but this man, neither at the time nor subsequently, felt any inconvenience from the experiment to which he submitted himself. Another, having spent a whole winter's night in the open air at a temperature of—60° C., is brought to the hospital with a rectal temperature of 24° C.; and within thirty-six hours he leaves the institution in perfect health (Nicolaysen). Conceding that the favourable issue of both these cases may in a great measure be attributed to individual peculiarities, still it shows not the less that the fatality generally ascribed to cold is greatly exaggerated. There is, of course, the unanimous assertion of almost all patients that they can distinctly trace their disease to the influence of that agent. But much of this opinion is, doubtless, due to the circumstance that the laity and physicians are accustomed to consider the presence of a bronchial affection, more especially, as a cogent proof that the patient *must have caught a cold*. And under these circumstances it is not difficult to recall the occasion when the cooling has taken place. Much, however, is attributable to this, that all inflammatory affections, whether slight or intense, local or constitutional, are ushered in, or are often for days preceded, by chilliness or rigors, while the causes of these (*i.e.*, the local changes) only subsequently appear. No disease is attended by more pronounced constitutional symptoms than pharyngitis; and here the febrile movement, cold followed by heat, is felt for days before the difficulty in swallowing is noticed. Thus the first expression of the disease is often taken for its cause; and this mistake will be readily intelligible from the disagreeable sensation which cold in general occasions.

Is then the economy of heat really disturbed so easily as supposed? Are the regulators of the temperature so powerless towards even a slight disturbance? Surely, the regard to the compensatory forces—the operation of which no organic processes exhibit in so striking a manner as those connected with animal heat—suggests a negative reply. Amidst the most varying external and bodily conditions, the healthy organism preserves its own daily temperature at an average height of 37.2° Cent. Within wide limits every loss of heat is compensated for by an increased production, and every excess of heat is rectified by an increased expenditure. This regulation proceeds from an excito-caloric and a moderating centre, to which thermal stimuli are probably conveyed from the periphery. But

it is carried out by the cutaneous capillaries, and here it is subject to the physical laws of temperature; cold contracts the vessels of the skin and forces the blood to the centre, or rather into the muscles; heat, on the contrary, dilates the vessels and favours perspiration.

It depends, therefore, upon the energy of the vital functions and the persistence and intensity of the disturbing element, how far the limits of the compensation reach. It is evident that those limits must necessarily vary in different individuals and at different times. Upon the whole, the body is better able to withstand cold than to dispose of its superabundant heat. The living organism produces, irrespective of food, two million calories in twenty-four hours; and this supply may be maintained or increased by various means, so as to meet every conceivable emergency. The difficulty at times is, as already mentioned, to remove the surplus, which, if accumulated in the body, would, in twenty-four hours, raise the temperature to the fatal degree of 48° C. The efforts in one direction are invigorating; those in the other have a debilitating effect. The local action of heat and cold presents the same difference, inasmuch as a slight increase of the temperature is destructive of the tissues, while even a considerable reduction of it merely renders their vitality latent. The frozen muscles of the frog retain, when thawed, their electrical irritability (Kühne). A comparatively slight increment of heat, on the contrary, coagulates the myosine of muscles and destroys their vital properties. Similar changes are observed in the blood; extreme degrees of cold are to the life of the red blood-corpuscles far less prejudicial than is even the febrile heat of the body. A rabbit's ear, may, with perfect impunity, be exposed to a temperature of —6° C. for from twenty-five to thirty minutes. Its exposure to a temperature of from + 46 to + 49° C. is attended by œdema of the surrounding tissues, and at + 50° C. the emigration of white blood-corpuscles commences. The reaction of a rabbit's ear naturally differs from that of a human lung, and the changes observed in the former allow no such direct inference as those that may occur in the latter. Still, these experiments justify the conclusion that the "cold" to which the lungs are exposed cannot possibly have the noxious consequences usually attributed to it.

Although the injurious influence of cold has been grossly exaggerated, yet it cannot be denied that asthmatics may suffer from dyspnœa, in consequence of a considerable loss of their temperature. Their organs of respiration and circulation would not only be greatly taxed, but might even fail to meet the consequent increased demand for oxygen. In what manner the pulmonary surface is, under such circumstances, affected is not

quite known. The face and hands of delicate individuals exposed to cold become cyanotic, because the arterioles are contracted, and the veins are gorged. But it is impossible that in the lungs the arteries can be so contracted or the veins so gorged as to produce the same degree of cyanosis. For not only is the inspired air warmed on its passage through the nose, but the bronchial surface is far more vascular than the skin, and much nearer to the centres of the production of heat.

Over-heating is a more potent and also a more frequent cause of dyspnœa than cold. It is well known that asthmatics greatly suffer in hot and sultry weather. Trousseau considers asthma as a disease most prevalent in hot climates and in the summer; but his observations are not quite to the point, inasmuch as they include also *hay* asthma, which he ascribes to heat and light, instead of the inhalation of pollen. The dyspnœa must be attributed to the imperfect disposal of the superabundant heat when the warm atmosphere is saturated with moisture. The pulmonary surface is probably then in a similar œdematous condition as the skin. The heated blood irritates the centre of respiration, and also that of the vagus (Cyon). The volume of the expired air increases, but the exhalation of carbonic acid diminishes, not on account of its deficient formation, but because the oxygen loses, in a ratio corresponding with the rise of temperature from 0° to 37° C., the power of expelling that gas.

Over-heating is not a less active cause of asthma in the winter season. There are numerous persons, in all stations of life, who at all times consume more than the necessary quantity of food, who, particularly in the winter, overburden themselves with warm clothing, and spend their time in hot and ill-ventilated rooms at the fireside. That food, and *a fortiori* an excess of it, as well as the products of the faulty digestion, do increase the bodily temperature, has been abundantly ascertained by exact thermometric measurements. Besides, the flushing of the face, the throbbing of the arteries, and the palpitation of the heart, from which those individuals generally suffer after meals, sufficiently indicate a febrile movement. Yet the disposal of their superabundant heat is a matter of considerable difficulty. The loss of the lungs is slight, because the air of the rooms is thoroughly warmed. The skin, even if no thick layer of subcutaneous fat impedes radiation—and the obese are the most numerous among that class of persons—is, on account of the great amount of clothing, surrounded by an atmosphere of almost the same temperature as that of the body. Among those who seek advice at hospitals, it is often seen that even the robust men are in the habit of wearing in the winter—1, a thick overcoat; 2, a frock coat; 3, a thick waistcoat; 4, a flannel shirt; 5, a flannel vest; and 6, a chest protector or "cat's skin,"

all these "to keep their chest warm." And the same anxious care is bestowed upon the clothing indoors. Under these circumstances Nature relieves itself by copious perspiration, and thus, by the evaporation of the perspired water, endeavours to reduce the temperature. The disagreeable sensations that are apt to arise from this cooling process, are regarded by the patient as a sure sign that "he must have somehow caught a fresh cold." And such is the vicious circle, that the means which he now adopts to combat only increases the evil. But although the regulators of the temperature succeed in preserving it for a length of time at its normal height, there is little doubt that they ultimately succumb to the prolonged strain upon their energy. This failure will not appear unexpected, if it be remembered that the mainsprings of that regulation are the cutaneous arterioles, or rather their muscular coats, whose continued relaxation would, even in the absence of any other cause, greatly derange their nutrition. If this much be admitted, it is needless to trace the steps that lead to the retention of heat in the body—a truly sub-febrile condition. Now, it is known that in insulation, death is due to the degeneration of all parenchymatous organs, and especially of the heart. The danger in acute infectious diseases arises from analogous causes. Experiments on animals, by exposing them to a high temperature for some time (36° C. for thirty-six hours), are attended by the same results. Not only the liver, the spleen, and the kidneys, but the entire muscular system, including the coats of the arterioles, undergo fatty or similar degeneration, these changes being then particularly marked in the substance of the heart. As such derangements of nutrition are invariably produced by a *high temperature* of only a *short duration*, it cannot be doubtful that the prolonged action of a *lesser* degree of heat must have similar consequences. Indeed, it appears that such is the case in those who most emphatically declare their susceptibility to "catching cold." One acquainted with their habits of life will readily concede that they seldom or never give *cold* the least chance of exerting its injurious influence, but that they suffer from the consequences of overheating. Their vessels are dilated and degenerated; serum, and even white blood-corpuscles, escape on the slightest provocation. A heated room, the irritating gases—the products of respiration and combustion, such as accumulate in the air of a theatre—and the dust which a current of air, *the draught*, raises from the ground; these suffice to produce the supposed effects of "*catching cold.*"—*Medical Press and Circular, May* 15, 22, 29, 1878, *pp.* 401, 423, 440.

DISEASES OF THE ORGANS OF DIGESTION.

22.—ON DIPHTHERIA, FROM A PREVENTIVE-MEDICINE POINT OF VIEW.

By Dr. W. N. THURSFIELD, S.Sc.C. Camb.

Dissemination of Diphtheria.—Space will not allow me to go into the question as to what constitutes the infection element in diphtheria, and in what way it may be connected with the low form of vegetable growth observable on the surfaces affected, but I shall confine myself to the ascertained modes in which this infection is spread and how it may be arrested.

It is my experience that the disease spreads solely through the ordinary channels of infection, and is not disseminated by means of mysterious atmospheric agencies, as is too frequently assumed. Diphtheria is, like other infectious diseases (perhaps more so), subject to an increased tendency to spread at one time more than another, owing to influences which, though probably connected with the individual, for want of a better name we call "cosmical." But, under ordinary circumstances, it has seemed to me that diphtheria is a very tangible and controllable infection, and that the distance through which the infecting agencies can travel through the air without becoming inert is very short. Of this I recently saw a striking example. Diphtheria was brought into a village, and, the first case being mild, grew into an epidemic before the village day-school was dissolved. In this village was a large school where pauper children from various unions were boarded and educated. This class is certainly not the least susceptible to infection in the community. Now, although several cases occurred within a few yards of this establishment, and a great many in the village, not a single case occurred in the boarding school.

The infecting element is probably given off in chief from the throat with the breath, or with the expectoration, and may thus get conveyed to the sewers; but, having regard to the observed pathological fact that the specific inflammation may, to a greater or less extent, spread over the digestive and even the urinary tracts, we need go no further to find a ready means of accounting for two modes in which the infection gets disseminated—namely, through specific contamination of sewers and of collections of excreta in latrines. Of both these modes of dissemination, especially the former, I have seen many marked instances. On one occasion, when called to investigate a case at a detached and perfectly isolated house in the country I found the patient had been to a neighbouring town, and had entered and been exposed to sewer gas in a house on a short line of sewer, which I knew had become specifically contaminated by

diphtheria. The owner of the property instructed a surveyor, residing some little distance off, to examine this sewer. He did so, and for that purpose had it opened, and was much exposed to the gas, and the second or third day after I received information that he was struck down with an attack of diphtheria, from which, and from its more remote sequelæ, he has suffered severely. As regards its dissemination by sewer gas, diphtheria has seemed to me to follow precisely the same laws as typhoid fever, especially in the fact that they both will attack with greater virulence and readiness new comers to an infected neighbourhood. From the latrines and sewers the *materies morbi* may readily, in some cases, obtain access to wells, which I believe may become specifically contaminated, though I have not absolutely proved it. In such cases the infection may be disseminated by the water, and therefore, like typhoid fever, by means of milk or any other fluid with which the milk may be mixed. Apart, however, from any question of admixture with impure water, having regard to the readiness with which milk absorbs volatile organic matter and the infecting material of certain diseases by mere exposure to an infected atmosphere in a confined room, and to the apparently tangible nature of the infecting elements in diphtheria, I believe that milk which had been kept in a house infected with diphtheria would form a ready mode of disseminating the disease.

The infection is most portable; this is a very important point which has generally been either overlooked or disputed altogether. I have no doubt about it whatever, and indeed I believe the infection of diphtheria to be as portable as that of any disease with which we are acquainted. I have known it taken to a house by a person living in an infected house, but not at any time suffering herself, but who went to visit a person a mile or two off, involving a considerable walk and a passage by ferry across a river, and only remained a short time in the house, but sufficiently long to leave the germ of diphtheria, which broke out a day or two afterwards. I have known it taken from one house to another a long distance off, and between which there was no other connection, or indeed any possible source of infection, by a woman who had simply been to fetch some yeast. Other remarkable instances I could give if necessary. The infection can certainly be taken from a corpse, or at least from its adjuncta; and, as in the case of small-pox, I believe it is specially liable to spread from such a source. I have known a coffin-maker take the infection home to his children after measuring a corpse, and another instance in which a woman who went to help to make shrouds contracted the infection. The infection will not only remain in unscavenged collections of excreta, and in the air of closed sewers, but will

attach itself to articles of clothing and furniture, and most persistently to wall-facings, and indeed seems to have a special predilection for so doing.

From the above it will be seen that, although the modes of dissemination of diphtheria are numerous, powerful, and subtle, there are none which do not admit of being controlled by disinfection carried out with precision. Of all diseases this is the one most liable to be spread by schools, and the reason, I think, is not far to seek, because the disease is frequently so mild as not to keep children from school, and may present no external indications. Moreover, the infection is peculiarly liable to be spread by the breath, and may indeed, I believe, remain, and be disseminated by the throat after children are sent back to school apparently well. I would suggest as a matter of ordinary practice that, as soon as any cases of infectious sore-throat have occurred in connection with a day school, that school should be closed for a sufficient number of days to cover the ordinary period of the incubation of the disease. For this purpose one week would be sufficient, provided the school walls and latrines are thoroughly cleansed, and the utmost caution observed in readmitting convalescents.

Origin of Diphtheria.—I wish to avoid expressing a belief in the *de novo* origin of disease, as that doctrine is commonly understood, but the arguments in favour of that view are far stronger in the case of diphtheria than in scarlatina or typhoid fever, and whilst, as regards the two last-named diseases, the number of cases in rural districts in which you cannot directly trace the source of infection is not more than would be anticipated on the theory of probabilities, with diphtheria the case is far otherwise, and all who have been called upon to investigate many outbreaks of diphtheria must have met with cases in isolated houses which seemed to admit of no other explanation. The researches which I have made in reference to these cases in old registers seem to me to show clearly that in all these cases, if we trace the habitations far enough back, we shall meet with previous outbreaks in the same house; and there are certain parts of the country where diphtheria may be said in this sense to be endemic, and where certain houses can be shown to have suffered repeated attacks at intervals of years. Now, it is my experience that there is one condition in common to all these places, needing only an exciting cause to bring and develop diphtheria. That this exciting cause need not be the importation of the specific infection of diphtheria I am confident, though if imported into such a locality diphtheria will flourish most vigorously. The immediate stimulus is to be found in the individual himself, and I believe under certain bodily conditions an ordinary cold is sufficient. I have repeatedly seen typhoid

fever imported into such a locality give rise to an outbreak of diphtheria instead of typhoid fever. I have, at least on one occasion, seen scarlatina do the same. I have also seen a convalescent from parturition develop and spread diphtheria, and I have once seen it follow an injury. The disease is infectious from the commencement of the outbreak, but the power of the infection appears, as I have pointed out previously, to be occasionally intensified by circumstances connected with the individual.

As typhus fever in its incidence and endemic form is connected with dirt and overcrowding, as in the same way enteric fever is connected with sewer gas, so diphtheria in its incidence and endemic prevalence is specially connected with dampness of habitations. Whether these results are from development of a specific disease on the application of a certain stimulus to a favourable soil, or from the infection of previous attacks, remaining dormant and revivified by the same stimulus, is a question easy to speculate upon, but difficult to decide.

Space, however, will not allow me now to pursue this subject further, which is perhaps all the better in a paper intended to be above all things practical.

The chief points I have wished to bring out are:—That diphtheria everywhere prevails inversely to typhoid fever, with which disease it is closely allied, although an attack of typhoid fever affords no protection from a subsequent attack of diphtheria, nor *vice versâ;* that when introduced into a town diphtheria may become epidemic, and be disseminated by precisely the same channels as typhoid fever; that the infection may be conveyed into any house by sewer gas, or otherwise, altogether irrespective of dampness of structure, but that its endemic breeding-grounds are to be found in certain well-defined spots in rural districts, where it is constantly liable to break out as if *de novo*, and that the constant condition of these localities is structural dampness of habitation.—*Lancet, August* 13, 1878, *p.* 210.

23.—ON THE TREATMENT OF DIPHTHERIA.

By Dr. ROBERT BELL, Physician to the Glasgow Institution for Diseases of Women and Children, &c.

In January 1876, it was my privilege to publish (*Retrospect*, Jan. to June, 1876, p. 47,) a short account of the plan of treating diphtheria, which I have adopted during the past few years. It is now my desire to detail the history of other cases of the disease which have passed under my notice since that date. It will be remembered that, under the treatment which was advocated, all the patients recovered, with the exception of

two, who proved themselves so obstinate that any attempt to carry out my wishes was frustrated. Since the publication of the paper referred to, I have met with at least eight well marked cases of this disease, all of which have recovered. I am induced to give an account of my further experience of this mode of treatment for many reasons, chief amongst these being that my medical brethren, who were in communication with me on the subject after the appearance of my former paper, may have further proof to support them in their endeavours to combat the disease by the means which have proved so successful in my hands.

Cases 1 and 2, M. F. and A. F., sister and brother, aged 10 years and 8 years respectively, were both prostrated on the same day. The film was spreading very rapidly over the tonsils, and there was considerable fever. The throat of each patient was freely painted every two hours with the following application: glycerine of carbolic acid, the strong liquor of the perchloride of iron, and sulphurous acid, of each three drachms; and the following mixture was given: chlorate of potash, two drachms; sulphurous acid, two and a half drachms; glycerine, one ounce; and water, to four ounces: two teaspoonfuls to be taken every two hours. In this way, while the patients were awake, an antiseptic was brought into contact with the diseased surfaces every hour. Port wine, soups, and milk were also given freely. The tonsils had only been painted a few times when the film showed signs of ceasing to spread; and within four days all the diphtheritic membrane had disappeared, and, saving a considerable amount of prostration, the patients were pretty well. A course of tonic treatment, however, soon removed this debility.

Case 3 was confined to one member of the family, but the disease was epidemic at the time. The patient was a girl thirteen years of age. The treatment as described above was adopted, and the disease ran very much the same course as in the two first mentioned cases.

Case 4 was the most intractable attack of diphtheria that I have seen. The patient was an only daughter, aged 16, and before her illness was a remarkably healthy looking girl. I saw her first on the 26th December last, and then considerable areas on both tonsils were covered with the fungoid deposit. The treatment was immediately commenced, and most rigorously carried out; but in this instance, it seemed for some time to have no control over the disease, for the false membrane continued to spread, till on looking into the throat nothing could be seen but the diphtheritic deposit. The treatment, however, was persevered in most persistently. Unfortunately, when the disease was about its worst, I became so unwell that I could not

leave my bed for a day, and a friend kindly took charge of the case for me, who saw that my instructions were most rigidly carried out. The father of the child, feeling desperate, asked to be permitted to see me in bed, to ascertain if nothing else could be done to relieve his daughter, my friend having looked upon the case as hopeless. I suggested a consultation, but the consultant said he could only confirm what my substitute had already said. Both, in fact, viewed the condition of the patient as being beyond hope of recovery. During all this time, be it remarked, the antiseptic application to the throat had the effect of preventing the poison from being absorbed into the general system, and on this account, I thought there were still reasonable grounds for hope. Having made the parents understand this, I urged them to persevere most conscientiously with the treatment, which they did; and with the assistance of my friend Dr. Brock, nothing was lost for want of a thorough application of the remedies. On January 3rd, the symptoms began to abate; and from that time, her recovery was uninterrupted.

Cases 5 and 6 were brother and sister, the only children of the family. Their history is very similar to that of Cases 1 and 2, and their recovery was quite as complete; though, in the girl, there still exists a great weakness of the throat, rendering her very prone to tonsillitis.

Cases 7 and 8 are very recently recovered from an attack of diphtheria. They are now quite rid of the disease, but are still suffering from the resulting weakness. They are likewise brother and sister, four and six years of age. It may be interesting to note that the laundry-maid in this family had an attack of diphtheria about eighteen months ago, from which she recovered under similar treatment.

Since the above was written, another child of the same household has been attacked, but is now on a fair way to recovery.

I would remark that, in every house where the above cases occurred, the arrangements for supplying the inmates with water were defective. In every instance, the drinking-water was drawn from a cistern, which was placed above the water-closet and bath-room; and when the cisterns were examined, they were found to contain an immense accumulation of slimy matter of a green colour, which under the microscope proved to be loaded with minute organisms, the greater number of which were of the same form as blood-corpuscles, and about the $\frac{1}{4000}$ of an inch in diameter. There can be little doubt that these microscopic bodies were the germs from which the disease sprang; and there can no estimate be formed of the immense amount of preventable zymotic disease which is constantly attacking the community under similar unsanitary conditions. It behoves us, then, as custodians of the public health, to insist

that such unscientific, nay criminal, modes of constructing houses be prevented.

P.S.—The above paper was written in the autumn of last year, and within the period which has elapsed since then, twelve other cases of diphtheria have come under my observation, all of whom have recovered under the same treatment. I may mention that five out of this number occurred in one family. Again the cistern water was at fault.—*British Medical Journal, May* 25, 1878, *p.* 749.

24.—ON TAPPING THE INTESTINE IN CERTAIN CASES OF INTESTINAL OBSTRUCTION.

By W. W. WAGSTAFFE, Esq., Senior Assistant-Surgeon to St. Thomas's Hospital.

[Mr. Wagstaffe gives us an interesting case of intestinal obstruction which ended fatally, but was relieved by tapping for the sake of letting out the wind, fæces also escaping. Commenting on this case Mr. Wagstaffe says:]

There remains the question of tapping as a means of treatment, either palliative or curative. Here was a case of intestinal obstruction threatening to end fatally, and for which the operation of abdominal section or colotomy were seriously considered and provisionally agreed upon, but which was relieved by what proved to be a simple and comparatively harmless treatment. If it afforded him permanent relief for so dangerous a malady can any rule be laid down for its employment?

Certain features about this case may assist in such an inquiry. This was a case of acute obstruction, suddenly occurring, although the cause had been existing for some time. The abdomen was consequently rapidly distended, the breathing was interfered with, and the heart quickly displaced. The removal of a quantity of gas was likely to give relief to lungs and heart, and to the over-distended gut; and it is in such cases that relief might be anticipated rather than in cases where, for weeks or months, obstruction has been slowly coming on from gradually increasing growth or accumulation.

And is it a serious matter to puncture a piece of intestine? If it contains gas only it is hardly likely to be, for gas only escapes, and as long as there is any distension the gas escapes externally. The simplest example of this is perhaps seen in cattle and sheep, when pricked for over-distension by wind.

A case of enormous distension by gas is reported by Dr. Martyn, of Bristol, where, after an attack of diarrhœa, obstruction occurred, with tenesmus and marked colic, but no vomiting apparently. On the fourth and sixth day he was tapped with a fine trocar, at first with the effect of relieving him slightly, the

second time, however, with the effect of relieving him altogether, for the action of the bowels was started and he recovered without any further complication. The cause of the obstruction was here doubtful; whatever it was, it was not apparently anything permanent, as in my own case. Probably it was due to the rapid decomposition of food producing tympanitis, for it followed diarrhœa, consequent, it was thought, on the patient's taking some fruit. But the clinical conditions were alike—acute distension by gas—and it is for such cases that the operation of tapping is to be urged.

I have put the question—Is tapping a dangerous operation? And I think it must be allowed that if gas only escapes, very little harm can arise, but if fæces bubble up, or only fæces appear, what then? Is it not likely to induce peritonitis? We have proof positive that it need not, from what happened in the case I have narrated. For here, three out of five tappings were almost purely fæcal, and yet, notwithstanding, the patient went out walking three days after, though it is probable he had some local peritonitis as a consequence.

Tapping cannot, however, be looked upon as altogether devoid of danger, for cases have undoubtedly occurred in which acute peritonitis and death have followed the operation, one being a case in which a strangulated hernia had been tapped after the plan recommended by Mr. Maunder.

I had occasion to tap the intestine in another case of intestinal obstruction (curiously enough sent to me by the doctor whose own case forms the subject of this paper), and here only very temporary relief followed. The cause of the obstruction was a scirrhus of the large intestine, and I eventually opened the ascending colon, but he died from rapid extension of the cancerous disease in the liver. At the post-mortem, no local effects of the tapping could be traced, although, as in the other case, some fæces had escaped from the punctures.

If fæces escape into the peritoneum inflammation more or less acute, must necessarily follow, and I have therefore adopted the following precaution during the operation. Before withdrawing the canula the end is carefully stopped with the finger, so that no air can get into the tube as it is being withdrawn. The absence of atmospheric pressure as the canula is passing from intestine through peritoneum to the abdominal walls, ensures, I think, the non-escape of the fæcal contents of the canula. After the removal of the trocar there is little tendency of the bowel contents to escape from the puncture, owing to the fact that the intestine contracts considerably, and there is very little peristaltic action immediately in the gut.

The aspirator is open to the objection that it is so often out of order, and also that a suction force is being used, sometimes to the injury of the parts operated on.

It seems to me that we are more than justified in tapping the intestine where it is over-distended with gas and the patient requires relief for his breathing or his heart, We are bound to adopt a measure which is not necessarily a serious one.

But can we expect to relieve an obstruction permanently by doing so? I think my case shows we can; and I may refer to a case narrated to me by my friend Dr. F. Taylor, of Guy's. He remembered a case in which tapping the intestine of its accumulated gas was followed by regular motions, although there had been chronic and slowly increasing but definite obstruction, with all its symptoms, before. The patient eventually died, and at the post-mortem a cancer of the large intestine was found almost entirely occluding the gut, but there was no evidence of ill effects from the puncture.

If mere puncture can permanently relieve, can its mechanism be explained? It seems that a satisfactory explanation of the phenomena is possible. It must, I think, be conceded that occlusion is rarely complete in cases of obstruction by growth or stricture—complete at all events to the extent of preventing the passage of fluid when the tube is examined post-mortem. In many cases the cause of the obstruction is a knuckly mass of scirrhus, where the upper part of the mass can be pressed down into the lower, very much like the lid of a box, or a conical plug, and the more it is pressed upon the tighter it is shut; or there may be adhesions binding down one part of the intestine, and over-distension of the gut above may throw the tube of the intestine out of a continuous line, and a fold or a bend in it may act just like a flap-valve, and the more it is pressed upon from above the tighter it will be closed.

If, however, pressure be removed from above, the course of fluid fæces will be continued along the original track. The removal of pressure may be by tapping gas, or by opening the gut more freely and letting out fæces, as in colotomy; and therefore, one would anticipate that sometimes after colotomy or enterotomy for permanent obstruction the passage of fæces by the natural channel would be re-established. This is certainly the case. It was so with the girl who formed the subject of my previous paper, and I have seen it over and over again.

If, therefore, it is comprehensible that so apparently simple a means of treatment may in certain cases of acute obstruction give permanent relief, we have, I think, another important weapon in our hands in the treatment of intestinal obstruction.

I would submit the following propositions with reference to the tapping of the intestine:

1. That there are cases of obstruction in which tympanitic distension of the abdomen is one of the most urgent symptoms.

2. That whether these be acute or chronic, relief is afforded by tapping with a fine trocar.

3. That if due precaution be taken no evil results, such as peritonitis, sloughing, septicæmia, &c., need be anticipated from the operation.

4. That especially in cases of rapid distension, where accumulation of fæces has not been going on for a lengthened period, good is likely to result from the operation.

5. That permanent relief may follow the operation to such an extent as to entitle it to be considered sometimes a curative means, obstruction being removed and not recurring afterwards.

6. That the mechanism of this relief is probably to be found in the removal of pressure above a valvular closure of the intestinal tube, rather than in the relief of an over-distended and, therefore, temporarily paralysed gut.

7. That the operation is worthy of more extensive application than it seems to have met with in this country.

Records of cases of intestinal tapping are not, I think, at all common, but I find in Dr. Hilton Fagge's exhaustive paper on intestinal obstruction, in the fourteenth volume of the Guy's Hospital Reports, three cases with the appearances post-mortem. In the first case (40)—one of chronic obstruction of the lower end of the small intestine by contractions from old peritonitis—the intestine was tapped on the sixteenth day, and a quantity of gas and a pint and a half of liquid fæces evacuated. The canula was left in for more than two hours, when it was obliged to be removed owing to the pain in the abdomen. She died on the thirty-fifth day and no mention is made of any mark on the bowel from the tapping.

In the second case (No. 55), one of twist of the ascending colon, admitted in a dying state, a trocar removed a quantity of gas, which gave much relief, afterwards some fæcal fluid followed, and the trocar was tied in till he died fifteen hours after. At the post-mortem there was no extravasation of fæces by the puncture.

In the third case (No. 56), also of twist, and probably of two weeks' duration, some air and then serous fluid came through the trocar. He died in about twelve hours. The post-mortem notes state that there was a spot of perforation, about a line and a half in diameter, in the centre of an ulceration, but there was no evidence that the trocar had entered here.

These cases, though perhaps not very positive in their evidence, are of some value in supporting the view that the operation is not necessarily a dangerous one.

Trousseau refers to the operation as one to be adopted when there is much distension by gas, but he does not suggest it as a

means of permanently relieving an obstruction. Still, his mention of the operation is, I think, some evidence of his belief in its comparative harmlessness.

Only within the last few years tapping the intestine in strangulated hernia has been recommended, and although I have not practised it myself, many cases have been recorded of its success. In a case, however, at St. Thomas's, in which it was done, there seemed very little doubt that it was the cause of fatal peritonitis.—*St. Thomas's Hospital Reports*, vol. viii, 1877, *p.* 38.

25.—THE TREATMENT OF ASCITES BY ABDOMINAL COMPRESSION.

By Dr. STEPHEN MACKENZIE, Assistant-Physician to the London Hospital.

There are numerous ways of treating the ascites dependent upon contraction of the liver. Alteratives, drastics, diuretics, tonics, compelling the patient to satiate his thirst from his peritoneal cavity, simply keeping the patient at rest in bed, all have their advocates, and are plans under which I, as every other physician, have seen good results in even unpromising cases. Every now and then, however, under any of these plans of treatment, alone or in combination, the peritoneal effusion remains stationary, and its removal becomes tedious. I have found that, in cases where there is any tendency to absorption, this may be greatly accelerated by a simple method—that of abdominal pressure—to be described, and that in cases in which absorption does not appear to be taking place the same plan is often successful in removing the fluid. I have employed this treatment in several cases, with such good results, that it encourages me to hope that others will find it useful. Perhaps I can best illustrate the plan of treatment I have found so successful in some cases of ascites, by narrating the first case in which I put it into operation.

E. S., aged 41, widow, was admitted into the London Hospital under my care on January 16th, 1875. The patient stated that she had not been thoroughly well since her last confinement, nineteen years previously. About one year before admission, in January, she noticed that her waistbands became tight. She attached no importance to the occurrence, however, at first. In February and March, she found that she was passing extremely small quantities of urine. In May, the size of the abdomen was much increased, and its "weight" inconvenienced her when she moved about. The enlargement of the abdomen increased rapidly up to June, when she attended at another hospital as an out-patient. She then measured forty-two inches

round the largest part of the abdomen. She attended for three months, and during that time increased in size, and continued to increase in size to the end of September, at about, she thinks, the rate of "an inch a month." Since September, the increase had been less marked, but had slowly continued, until coming under treatment. She had only passed very small quantities of urine. She was the mother of two children, and had miscarried once, owing, she thought, to a strain. There was nothing in her history to support the diagnosis of syphilis; no intemperance could be elicited on close questioning. She had never had jaundice. When admitted, I noted that the patient was much emaciated, but was not decidedly cachectic. The abdomen was greatly distended, of ovoid shape, tense, with extremely distinct fluctuation-wave. The circumference of the abdomen at the umbilicus was forty-six inches, and the same two inches higher. Medium-sized superficial veins were seen coursing over the abdominal walls from below upwards, as could be demonstrated by pressure. There were no veins passing downwards to the thighs; the umbilicus protruded slightly. There was œdema of the lower extremities, from the thighs to the feet, especially on the left side. There was a small patch of discoloration on the inner aspect of the left leg, which, she stated, had been in existence for five years. It came as a small speck of the same colour, and has gradually increased to its present size. There was no history of injury to the part; no nodes. Hepatic dulness in the right mammary line was bounded by the upper border of the fourth rib. Here and in the axilla it was about two inches in extent, and posteriorly on a level with the twelfth dorsal spine, it was four inches deep. The chest organs were pushed up, but presented no noteworthy signs. Vaginal examination showed the uterus and anterior wall of the vagina depressed (cystocele).

[The state of the patient continued variable until May 11, when the report is]

The condition of the patient appeared stationary. There was still a considerable amount of fluid in the peritoneal cavity. She passed about forty ounces of urine in the course of the twenty-four hours. *The abdomen was ordered to be tightly bound* with a flannel bandage, so as to exercise pressure.

June 4th. When the bandage was first applied to the abdomen the pressure caused a feeling of sickness, but soon the patient bore it without discomfort; in fact, it appeared to afford her relief. The measurement around the abdomen, at the level of the umbilicus, was now thirty-three inches. She passed about thirty to forty ounces of urine daily, and did not perspire much. I now ordered a properly made abdominal

support to be constructed for her by the instrument-maker. It was arranged with straps, so that it could be tightened to a very considerable degree.

June 29th. The patient expressed herself, and appeared to be, in good general health, but she was still much wasted. The abdomen looked much smaller, and measured thirty-one and a half inches at the level of the umbilicus. There was pseudo-fluctuation on percussion, but it did not convey the impression of being due to fluid. The liver could be felt in the same situation as before. Its surface was smooth, its edge firm, sharp, and regular. The spleen was not to be felt in the abdomen. Its dulness reached the seventh rib in the axillary line. She passed, on an average, fifty to sixty ounces of normal urine daily. She ate and slept well. She was now allowed to get up, but was directed to continue wearing the abdominal support. The latter, owing to the diminution of the size of the abdomen, had required to be altered. After being up in the ward for a few days, the patient was allowed to go into the garden; and there then being no reaccumulation of fluid in the abdomen, she was made an out-patient. This was in July 1875. The patient has been continually under my observation from then until now. She has had no recurrence of the ascites. Her general condition of health is feeble, but she is able to do her household work and earn her living. Unusual exertion causes slight œdema of the feet. She has never had albuminuria.

Another case where equally good results followed, is the following, A. T., aged 46, married, was admitted into the London Hospital under my care September 3rd, 1875. She was quite well until two years before admission, when her feet began to swell. The catamenia, hitherto regular, at this time ceased. After the feet had been swollen two or three months, the abdomen began to swell; and it became so large that she came into the hospital eighteen months ago. She remained in the hospital three months, and during this time was twice tapped. On returning home, the swelling increased again, and she was again tapped. After this, the fluid again accumulated; she was re-admitted into the hospital and tapped again. She improved, and continued as an out-patient until about eight weeks before her present admission, when her abdomen began to swell again, and it gradually increased, until she came into the hospital. There was nothing in her history to suggest syphilis; no excessive indulgence in alcohol could be elicited. The patient was a stout person, with injected cheeks, puffy face, watery and glistening conjunctivæ. The cardiac dulness could not be mapped out; the impulse was indistinct; the sounds were natural; the lungs were normal except for slight emphysema.

The abdomen was much distended, of spherical form, rising three or four inches above the level of the sternum, and measured forty-seven and a half inches in circumference at the umbilicus. Fluctuation was distinct; the superficial abdominal veins were somewhat distended. The liver dulness appeared to reach the lower border of the fourth rib, in the right mammary line. Neither liver nor spleen could be felt in the abdomen. There was slight pitting on pressure over the legs. The urine was scanty, acid, of specific gravity 1,012, free from albumen. The temperature, pulse, and respiration were normal. The abdomen was directed to be bandaged with a wide flannel roller. She was ordered to take mistura rubra, ℥i., three times a-day.

November 15th. The ascites was considerably diminished. The abdominal walls were quite flaccid and and thin. The liver-dulness did not extend two fingers-breadth in the right mammary line. The general condition was improved.

December 6th. The abdomen was greatly diminished in size; it was flaccid and pendulous. There was no distinct fluctuation wave now, but there was dulness in both flanks, whilst the summit of the abdomen was resonant. There was still slight pitting over the feet and legs. She was directed to continue the abdominal pressure.

December 20th. The patient was now up; the abdomen had not enlarged since she had left her bed. There was no œdema of the feet. She still had the kidney look observed on admission. For the last two months, she had passed, on an average, sixty-five ounces of urine daily, of pale colour, specific gravity varying between 1,010 and 1,015, and, though repeatedly examined, it had never contained albumen. The patient was now allowed to go home and attend as an out-patient. Whilst she continued under observation there was no recurrence of the ascites. The only medicine she took throughout her residence in the hospital was the mistura rubra of the London Hospital Pharmacopœia (burnt sugar and water).

Hitherto I have not tried abdominal compression in *increasing* ascitic effusions. It has seemed to me that the abdominal walls offered as much opposition to the pouring out of fluid as it would be safe to call into play, and that a greater strain on the vessels might lead to their rupture—effects we not unfrequently witness in cirrhosis, as hæmatemesis and hemorrhage from the bowels. It is, however, quite possible that the obstacle offered to the effusion from the portal veins may be safely augmented to such a degree as to force open the anastomoses of the portal vein, and, by thus effectually carrying off the blood brought to the liver, to *cure the disease*. I use the word cure advisedly, as fibrosis of the liver in the main kills the patient by the secondary effects of the mechanical obstruction of the portal circulation.

A person can get along, by management, with half a liver. In contraction of the liver, if the blood which cannot traverse the organ be efficiently carried off by other routes, and is not lost to the system, enough blood may yet pass through the liver to enable this large gland to perform its varied and important functions *for all ordinary needs* of the body. So far as I know, the plan of abdominal compression has not been recommended for cases of ascites, though I make no claim for its novelty. It has proved useful in my hands, and, I believe, will be found serviceable in causing or accelerating the absorption of an ascites, alone or in conjunction with other methods of treatment.—*British Medical Journal, April* 20, 1878, *p.* 563.

DISEASES OF THE URINARY ORGANS.

26.—ON SOME POINTS CONNECTED WITH DIABETES.

By Dr. F. W. Pavy, F.R.S., Physician to Guy's Hospital.

It is a minimum amount of sugar in the blood which is the character of health, and a larger amount which is associated with unnatural states. To maintain the minimum amount the liver exercises a sugar-detaining and sugar-assimilating function, and this prevents us from being diabetic. It has been asserted by Bernard, and my own observations confirm the assertion, that the blood of the herbivora contains no more sugar than that of the carnivora. Indeed, according to the results I have furnished in a former part of these lectures, the blood of the dog contains a higher proportion of sugar than that of the sheep or bullock. Now in the herbivora, besides the sugar derivable from an internal source, we have that to deal with proceeding from ingestion; and if the liver did not stop, as I contend it does, the passage of sugar into the general circulation, there ought to exist a marked difference adversely to what is noticed. The sugar which actually escapes from the liver is so small in amount that I cannot conceive it to have anything to do with the large operation of appropriation of this principle to force, production that must occur as a part of the general phenomena of life.

Expressed in precise language, I would say that the liver is essentially a sugar-assimilating instead of a sugar-forming organ; and that when its assimilative action is properly exerted, so little sugar is allowed to pass into the general circulation that the quantity existing in arterial blood is insufficient for rendering the urine more appreciably saccharine than is observed in the healthy state; but that when its assimilative action is not properly exerted, sugar is allowed to pass, and in proportion as it does so the urine acquires a more or less marked saccharine character.

The question before us is one which resolves itself into the utilisation or non-utilisation of a certain kind—viz., the carbo-hydrate kind—of material. We know that in diabetes this material reaches the blood as sugar, and thence escapes unutilised with the urine. If in health it passed in a similar manner into the blood, to obviate its escape it must undergo appropriation in the circulatory system, and the difference between health and diabetes would then resolve itself into a difference in the capacity of appropriation within the circulatory system. This view, however, is not adopted by the advocates of the glycogenic doctrine, and the evidence afforded by observation is against it. We are therefore driven to the conclusion that the material in question must have a different destination under normal circumstances, and I contend that in reality the liver ought to detain and assimilate it instead of allowing it to pass as sugar into the circulatory system, and that it is by this action we escape being diabetic. Put in another way, it may be said that the sugar escaping in diabetes is the representation of the carbo-hydrate material which ought to be utilised in the system, but which, finding its way into the general circulation as sugar, is thence eliminated as it is. To escape being thus eliminated, it is obvious that the carbo-hydrate material in question must pursue some other path towards utilisation than passage into the general circulation as sugar, and this is tantamount to saying that under normal conditions there must exist a capacity of appropriating carbo-hydrate material in this other direction equal to the disposal of that which is represented by the sugar escaping in a full-marked case of diabetes, and this embraces, if not all, certainly in the main, all the carbo-hydrate material which the system has to deal with. Not only, therefore, must there be this other power of disposal, but such power must be exercised, instead of the material being allowed to pass into the general system as sugar, as is represented under the glycogenic doctrine, to avert the manifestation of diabetes.

I have previously discussed what Bernard has said about the appropriation or destruction of sugar in the circulatory system, and have shown that my own analytical observations give no evidence of any significant amount of destruction occurring. It may be assumed—as, indeed, the aggregate of my results indicated—that a minute disappearance takes place. With an organic principle like sugar, susceptible of undergoing change such as occurs in the lactic acid fermentation, it is scarcely to be expected that it would remain uninfluenced by the molecular actions going on around it; but that nothing of a nature to be compatible with the large disposal of it as an alimentary principle that must ensue is to be recognised is, I consider, proved

by the evidence that has been already adduced. If the destruction of sugar occurred within the circulatory system of the herbivorous animal to an extent sufficient to account for the disposal of that which is derived from ingestion as well as from alleged formation, what, it may be asked, ought to be the condition in the carnivorous animal which does not receive the supply of sugar from without? Surely, if the destruction is carried out with sufficient fulness to keep down the sugar in the system of the herbivorous animal, it ought to be equal to the entire removal of the sugar contained in the arterial blood during its passage to the veins in the case of the carnivorous animal. In reality, the sugar that has to be dealt with in the blood shows itself to be as much beyond the capacity of destruction in the peripheral vessels in the carnivorous as it is in the herbivorous animal. No material difference, in fact, is to be traced in the state of things in the two groups of animals. In both cases alike the sugar contained in arterial blood reaches the veins without undergoing any material diminution, and thence must needs circulate through the system over and over again. Whatever destruction occurs can only fulfil a subsidiary office. Admitting that the operation is to some extent carried on, it is altogether wanting in the magnitude required to be associated with the appropriation of sugar as a force-producing agent in the body.

My own version of what occurs is this. We know that in a large number of animals the food is of such a nature as to supply sugar in notable quantity for absorption from the alimentary canal. A portion of this may reach the thoracic duct through the absorbents, and thence be conveyed to the general circulation, accounting in part for what sugar is there encountered. The main channel, however, for the passage of sugar from the alimentary canal appears to be the bloodvessels, and transmission into them is permitted by the property of diffusibility which this agent possesses. Absorbed into the portal system, it is conveyed to the liver, where it becomes, if not entirely, certainly almost entirely, checked in its onward progress, and prevented from entering the general circulation. I have shown, by observations which have been for a long time made known, and which I need therefore here only allude to, that sugar leads to an increased accumulation of amyloid substance in the liver. The evidence adducible points to the occurrence of a direct formation of amyloid substance from absorbed sugar. As the sugar is passing through the capillaries of the organ it becomes picked out by its cells and converted into the principle I have mentioned. Nothing could be more favourable than the conditions that exist for the occurrence of this operation. Surrounded as the capillary vessels of the lobule are with rows of

secreting cells, the blood is brought into the closest proximity with the secreting element, and thus most advantageously placed for selective action to be exerted.

Micro-chemical observation shows that the amyloid substance of the liver is confined to the cells, and doubtless immediately the sugar is abstracted from the blood it is transformed by the action of the cell into the principle named, for otherwise the organ would give evidence of the presence of sugar in a manner that it is not found to do. Moreover, sugar is a diffusible substance, and would not therefore be retained. Amyloid substance, on the other hand, belongs to the class of colloids, and with its non-diffusibility possesses the properties which physically contribute to its retention in the cells, where it presumably undergoes a change which forms one of the links in the series leading up to the final issue—the utilisation of sugar as a force-producing agent in the system.

As regards, then, the sugar derived either directly or indirectly from the food and absorbed from the alimentary canal, my proposition is that under normal circumstances it is stopped by the selective or secreting action of the cells of the liver, and in these transformed into amyloid substance. When not thus stopped it reaches the general circulation, and as a result gives rise to a saccharine impregnation of the urine, standing in proportion to the amount of sugar absorbed from the alimentary canal. This is just the condition that exists in diabetes, and it is well known that in this disease the eliminated sugar stands in relation to the amount of sugar or sugar-forming material ingested.

But it is not only from the sugar derived from the food that the amyloid substance of which I have spoken takes origin. I do not know of any evidence to show that it is formed from fat. Nitrogenous matter, however, undoubtedly is a source for it. This is proved by its existence in well-marked quantity in the liver of the animal-feeder kept upon lean meat. In diabetes also, where a severe form of the complaint exists, sugar is voided upon a strictly animal diet, and such sugar may be put down as taking origin from the abnormal descent of the amyloid substance derived from nitrogenous matter. It is known that injested nitrogenous matter leads to the production and elimination of urea, and it is believed that the production occurs in the liver. A splitting up of the nitrogenous compound ensues, and its nitrogen, appropriating a certain amount of its carbon, hydrogen, and oxygen to form urea, leaves a complementary portion of carbon, hydrogen, and oxygen, from which it may be assumed amyloid substance is generated. If we take the percentage composition of albumen and give to the nitrogen the amounts of the other elements required to form urea, we see

the amounts of carbon, hydrogen and oxygen that may be applied to the purpose named. This table affords a view of how the matter stands:—

Urea and Complementary Residue derivable from Albuminous Matter.

	Albumen.		Urea.		Residue.
Carbon	53·50	..	6·64	..	46·86
Hydrogen	7·00	..	2·21	..	4·79
Nitrogen	15·50	..	15·50	..	—
Oxygen	22·00	..	8·85	..	13·15
Sulphur	1·60	..	—	..	1·60
Phosphorus	0·40	..	—	..	0·40
	100·00		33·20		66·80

From this tabular representation it is seen that in the presumed splitting up of the albuminous molecule one-third passes into urea and two-thirds into complementary residue—that is, the relation between the urea and complementary residue is as 1 to 2. Now, some observations by Dr. Sydney Ringer on the relation between eliminated urea and sugar in diabetes that were published in the Medico-Chirurgical Transactions for 1860 present a conformity which is certainly striking, if nothing more. The observations show that during fasting and under a diet of animal food—when, in other words, there is no sugar from ingestion to influence the state of the urine—the eliminated urea and sugar stand in the ratio of 1 to 2·2. As might be expected, the figures representative of the eliminated products are much higher under the animal diet than during fasting, but notwithstanding this difference, the same ratio was maintained. It is right to state that this correspondence I have brought into view must not be taken for more than it is worth, inasmuch as the complementary residue referred to does not express the composition of sugar or of one of the allied carbo-hydrates, the carbon being out of proportion to the hydrogen and oxygen, and particularly so to the latter.

Whatever may be the precise manner, however, in which the elements of nitrogenous matter become rearranged, it may be said that there are grounds, on the one hand, which have led observers to entertain the view that it is in the liver the production of urea occurs, and practically, on the other hand, it can, as I have mentioned, be shown that from nitrogenous matter amyloid substance is here formed. It is the latter point which essentially concerns us with reference to diabetes. The liver not only arrests the passage of sugar absorbed from the alimentary canal, and effects its transformation into amyloid substance, but also forms this substance from nitrogenous

matter. This may be regarded as constituting the first step of an assimilative action exerted by the organ; and, if we are not able at present to follow the process on, and state what next occurs, we can, I consider, say upon the evidence I have advanced that the amyloid substance is not physiologically destined to undergo conversion into sugar, and pass as such into the general circulation. The glycogenic doctrine, it is true, implies the occurrence of such an event. Transformation into sugar is contended for, and Bernard speaks of this formation of sugar as the beginning of a series of phenomena of combustion ultimately resulting in carbonic acid and water.

The problem to be solved is the nature and seat of the lesion of the nervous system, which forms the primary morbid condition in diabetes. There are various considerations connected with the disease which point to the brain as being the most likely part from which the morbid influence starts. We know that by operating upon the medulla oblongata and the sympathetic system diabetes may be artificially induced; but it is not necessary that attention should be confined to these structures, for it is also known that in them only resides a part of the vaso-motor system. The observations of Eulenberg, which are corroborated by those of Brown-Séquard, have shown that the state of the arteries is affected by lesion of certain parts of the grey matter of the brain, and it is suggested that the vaso-motor nerves distributed in the sympathetic, besides being connected with the spinal cord and the medulla oblongata, pass up to spots at the surface of the brain, which stand in the position of cerebral vaso-motor centres.

I certainly incline to the opinion that some kind of textural change in the brain stands at the foundation of diabetes, and there are two ways in which the effect may be produced. The vaso-motor system exerts an influence upon the arteries, which gives them their *tonus*, or keeps them in a certain state of contraction. The effect of destruction of the centres or tracts is to lead to arterial dilatation, by causing direct paralysis of the muscular coat; whilst that of irritation is the converse. These results are in harmony with what occurs under similar circumstances in connexion with the cerebro-spinal system, and are therefore intelligible enough to us. Arterial dilatation, however, may be induced in another way—viz., by an action of the cerebro-spinal system controlling or inhibiting the activity of the vaso-motor system. Reference to what may be perceived in connexion with the salivary glands will supply us with an illustration of the occurrence of this mode of dilatation.

Whilst the glands are in a state of quiescence, the arteries are maintained in a comparatively contracted condition by the influence of the vaso-motor nerves they receive from the sympa-

thetic. Now common observation shows us that a flow of saliva, which means an antecedent dilatation of the arteries, may be excited by a stimulus applied to the surface of the tongue, and so influencing the extremities of the lingual gustatory nerve, and even by an impression starting from the brain and originating in a passing thought about food. The explanation which physiological science gives of this phenomenon is that a stimulus is transmitted from the cerebro-spinal system which produces an inhibitory action upon vaso-motor centres or nerves that relaxes the muscular coat of the arteries, and thus permits dilatation to occur. This may be considered to constitute an active form of vaso-dilatation, in contradistinction to that arising from paralysis induced by operations upon the sympathetic, which lead to a simple removal of vaso-motor influence.

There being these two modes of action by which vaso-dilatation may be brought about, it may happen that diabetes may arise either from a lesion affecting and involving a loss of power in vaso-motor centres, or a lesion in some part or other of the cerebro-spinal system leading to an inhibitory influence being exerted upon them.

Dr. Dickinson states that he has recognised certain vascular and perivascular changes in the brains of those who have died of diabetes. My colleagues Drs. Frederick Taylor and Goodhart, however, in an article "On the Nervous System in Diabetes," published in the Guy's Hospital Reports for 1877, have disputed the validity of his conclusions. The point in question is out of my own line of investigation, and I cannot therefore pretend to offer an opinion upon it; but this much I may say, that I hope in the interest of science Dr. Dickinson will continue and extend the inquiries he has begun, for I am sure it will be admitted that he possesses qualifications which especially fit him for such work. The ground I consider has been prepared by physiology, and we must now look for the fruits derivable from the assistance of pathology.

I may state, and I desire that the statement should only stand for what it may appear to be worth, that my clinical experience has led me to form the idea that diabetes presents an alliance as regards the character of its progress to locomotor ataxia and progressive muscular atrophy. We have in each of the cases different manifestations of disordered nerve-action to deal with; but, as in the diseases named, diabetes, certainly as it occurs among young subjects, is a truly progressive affection. For instance, when such a subject falls under observation during the early period of the disease, it will be found that by dietetic management alone the urine may be deprived of sugar and the health restored. Later on sugar reappears notwithstanding the most rigorous dietetic management be persistently adopted.

It may happen now that the administration of opium, morphia, or codeia will again remove the sugar. Again, however, the urine becomes saccharine, and the saccharine character gradually augments. With this the patient notices that he is losing ground in general health. He experiences more or less of a return of the old symptoms which troubled him at the outset before his eyes were opened to the nature of his complaint. No impression now can be made upon the disease as formerly, and it appears to me we can only assume that the pathological condition which is at the bottom of it must have gradually advanced. The ordinary duration in these progressive cases, I think I may say, is about two years. Sometimes it is much shorter, sometimes longer; and, as in locomotor ataxia, the condition may advance to a certain point, and there remain stationary for a shorter or longer while, the patient during this time being able by strict attention to treatment to keep himself in a fair state of health. These remarks do not apply to the disease in elderly persons, for here, if the proper management be pursued, the tendency is towards progress in a right direction, if not even recovery, instead of towards an unfavourable issue.—*Lancet, Aug. 3 and Sept.* 21, 1878, *pp.* 143, 393.

SURGERY.

AMPUTATIONS, FRACTURES, DISLOCATIONS, AND DISEASES OF THE BONES, JOINTS, ETC.

27.—ON SUPRA-CONDYLOID AMPUTATION OF THE THIGH BY THE METHOD OF GRITTI, AS MODIFIED BY STOKES.

By W. H. A. JACOBSON, Esq., Assistant-Surgeon to Guy's Hospital.

The supra-condyloid amputation by the method of Stokes has not received sufficient attention in England. It is often confused with that amputation which goes by the name of Gritti. Between the two there is this all-important difference; in Gritti's the section of the femur is made through the condyles, in Stokes' at least half an inch above them, in other words the one operation is trans- the other supra-condyloid. On this point great stress has been laid by Mr. Stokes, and a comparison of the two operations will convince every one that he is right. If the section of the femur be made through the condyles the patella will either be drawn up altogether on to the front of the femur, or else will project forwards, somewhat like the half open lid of a box, at an angle to the broad sawn surface, the whole of which it is impossible that it can cover, and across which it is liable at first to be shifted by the contraction of the vasti after it has been got into its place, while an amount of force will be required to get it into its place, which is almost certain to result in bruising of the cut periosteum on the edge of the femur, and consequent necrosis of a small shell of bone as happened in my second case, where sufficient attention was not paid to this point. If on the other hand the saw is made to pass half an inch above the condyles, the patella will drop easily over, and cover completely the sawn surface of the femur.

Of the advantages over amputation through the thigh which the supra-condyloid amputation of Stokes possesses, some are common to it and amputation through the knee-joint, while others are peculiar to itself, of the former the most important are—

1. The patient can bear his weight in walking on the face of

a shapely stump, thus he is not compelled to take his bearing from the tuberosity of the ischium, and to walk as if he had an ankylosed hip-joint (Stokes), as is the case after amputation through the thigh, where the stump is ever liable to be fretted by the slightest pressure.

2. There is less risk of shock because—*a.* the limb is removed farther from the trunk—*b.* the section of the muscles is made not through their vascular and fleshy bellies, but through their tendons.

3. Only one large artery requires ligature or torsion.

4. The medullary canal is not opened, and on this account there is less risk of osteomyelitis and the formation of sequestra.

5. Very good power of adduction is left over the artificial limb. Every surgeon must have noticed how badly off a patient is in this respect after ordinary amputation through the thigh. In this method the adductors are left almost intact, even to part of the vertical strong tendon of the adductor magnus, the result being that the balance between the abductors and adductors of the femur is left practically undisturbed, and that the patient when walking, has none of that difficulty in bringing his swung-forward limb in again under the centre of gravity of his trunk, which is seen after amputations higher up.

Amongst the special advantages of this method may be reckoned—

1. The normal attachment of the extensor muscle is not only preserved, but from the bony ankylosis which occurs between the femur and the patella, there is a fixed point for the extensors to pull upon. (Stokes.)

2. Healing takes place quickly, for (*a*) there is no exfoliation of cartilage to take place, and (*b*) there is no sloughing of the flaps to be apprehended. It is well known that in the ordinary amputation through the knee-joint by a long anterior flap going five inches below the patella, sloughing is not at all uncommon; a reference to the anatomy of the arteries of the part will show that while both these amputations have the merit of retaining the patella, and so of not interfering with the arterial anastomoses about it, the line of the incision (reaching to the tubercle of the tibia) by which the anterior flap in supra-condyloid amputation is cut, passes through arteries which are running from above downwards, viz., the anastomotic, the descending branch of the external circumflex and superior articular; in amputating through the knee-joint, on the other hand, the line of incision (which reaches to three inches below the tubercle) passes through vessels which are running from below upwards, the inferior articular and the anterior tibial recurrent, and that thus the blood supply to the flap is materially interfered with.

3. The stump is much more easily fitted with an artificial limb. Instrument makers, in many cases at least, have not found it easy to fit an artificial leg to the stump which is left after amputation through the knee-joint. Thus, Mr. Stokes quotes Mr. Heather Bigg's remarks that although the retention of the patella and consequent ability of the patient to take a firm and efficient bearing on the end of the stump offer many advantages, yet they are neutralised by the circumstance that the presence of so long a stump interferes with the shape of the prothetic mechanism, it being absolutely impossible to give a natural representation of the knee-joint when so large a portion of substance remains to be disposed of. Also "from the length of the stump requiring a resting surface, that surface would project beyond the other limb on the knee being fixed, giving the wearer an appearance of possessing one leg longer than the other." No such difficulty has been met with by Mr. Stokes in his nine successful cases of supra-condyloid amputation. Moreover, Mr. Millikin tells me, "For instrument makers this is a very desirable operation, as it gives plenty of room for the knee-joint, and allows the patient to take good weight on the end of his stump; although after the usual amputation through the knee-joint the patient is able at first to bear on the face of his stump, he very soon in most cases has to take his weight on the upper part of the thigh, as there is a large surface of bone with a very slight covering." Out of eighteen cases which have come under his notice only five have, after wearing an artificial leg for three months, been able to continue to take their weight on the end of their stump.

With regard to the disadvantages of this operation, I attach no importance to the fact that it entails more time and trouble than an ordinary amputation through the thigh. At first sight a real objection presents itself in the tendency of the patella to be drawn off the sawn face of the femur by the extensor muscles in certain cases. This tendency is naturally much more marked in primary amputations in strong healthy subjects. In such cases it may be met in one of two ways—(*a*) by turning up the under surface of the flap and dividing the rectus tendon, (*b*) as practised by Mr. Stokes by passing a needle threaded with cat-gut through the tissues immediately adjacent to the posterior margin of the femur, and through those immediately above the centre of the inferior edge of the patella, and leaving the ligatures cut short in the wound. The retention of the patella in its place will be much facilitated by paying attention to the exact site at which the saw is made to pass through the femur for the reasons already given.

With regard to the cases to which this method is applicable it will be borne in mind that the limit of the anterior flap is the

tubercle of the tibia, *i.e.* three inches short of that made in the ordinary amputation through the knee-joint. Speaking generally, it may be used in those cases in which amputation at "the seat of election," or through the knee-joint is not possible, but where the surgeon prefers a supra-condyloid amputation to one through the thigh.

Amongst such cases will be—

1. Fractures of the leg calling for amputation in which the injury to the soft parts extends high up.
2. Extensive necrosis, or abscess of the head or upper third of the tibia.
3. Certain cases of growth in which the removal of the whole leg is expedient.
4. It is perhaps doubtful how far this method will admit of being made use of in those cases of disease of the knee-joint which call for amputation. As regards the condition of the femur at the time of the operation, or the probability of any return afterwards I quite believe that the operation may be employed in these cases, and, no less than three inches of the femur having been removed, that there is no risk of any return of disease in this bone.

In the case of the patella it is different, as the probability of recurrence of mischief is greater. With reference to this point I cannot do better than quote from a letter, for which I am indebted to the courtesy of Mr. Stokes, who has himself employed this method successfully in a case of necrosis of the tibia with disease of the knee-joint commencing in the synovial membrane.

"Though the operation is doubtless best adapted for cases where the injury or disease does not involve the knee-joint, there are conditions such as for example, articular ulceration, where if there were contra-indications to resection, I should certainly prefer supra-condyloid to any higher amputation."

Finally, the object of this paper will be attained if it helps to secure for this method of amputation a wider trial; that it is entitled to this is shown by the facts that it has been practised by Mr. Stokes in ten cases, in nine with entire success, and that it is very well thought of by surgeons of such high standing as those at the Leeds Infirmary.—*Guy's Hosp. Reps.* 1878, *p.* 211.

28.—ON THE EXCISION OF PHALANGEAL JOINTS.

By ENGLEDUE PRIDEAUX, Esq., late Resident Surgeon at the Scarborough Dispensary and Accident Hospital.

It may be thought that the subject of this note is of very little importance; but we should attach almost as much importance to the best modes of treating minor complaints as to those

of treating major ones, and although the fingers are comparatively small parts of the human body, they are relatively very useful and important ones, and should not be neglected. A stiff finger to a labouring man, especially when engaged in certain occupations, often means inability to earn his livelihood, and after an injury to his finger almost necessitating amputation, when he begs to have it left, and makes light of its probable stiffness, that result often causes him to return to have it removed after all, as he finds it would be better to be without it than let it impede him in his daily labour.

No doubt this consideration affects those cases which so often occur of the amputation of a finger for joint disease. It is true, if the finger be left to itself, after a time, longer or shorter as the case may be, anchylosis may take place, but it is after a time during which the possessor has been incapable probably of earning his living, whilst in his attempt to do so the finger has received all kinds of rough treatment, its condition thereby being aggravated. The result, after this long and painful waiting, is a finger utterly useless in most cases to its owner; and one cannot wonder, looking at these circumstances, that it is deemed better to amputate.

Now what I want to call attention to is that there is a far better plan than either of these, that results, in a comparatively short time, in a finger slightly shortened, but with a good movable joint, thereby saving a very useful member, and preventing the deformity that ensues from amputation: I refer to excision of the affected joint. Resection of the finger-joint is as applicable and as useful in proportion as resection of the knee-joint, and yet how often does one see what might be made a good and useful finger amputated for disease of one of the phalangeal joints, simply, I think, because attention has not been called to the preferable way of treating them by excision, and it does not occur to those who amputate. It may be considered a very trivial matter, but every detail in surgery has its importance, and cannot afford to be neglected; and to those whose fingers are the means of their livelihood the question of amputation or excision is to them often a matter of pounds, shillings, and pence. In the majority of the text-books of surgery I find no mention of the subject. Erichsen says, "Disease of the phalangeal articulations usually leads to amputation of the affected finger." Bryant, in his "Practice of Surgery," mentions excision of the phalangeal joint in the thumb as leading to good results, but does not mention the fingers, except in directions to obtain anchylosis with a curved finger instead of a straight one.

The operation for resection of a phalangeal joint is very simple. A vertical incision over the dorsal surface of the joint should be made, taking care to avoid the extensor tendon; then

the heads of both bones should be exposed, and cut off cleanly with a pair of cutting bone-forceps, taking care to injure the extensor tendon as little as possible; the wound should be then closed with sutures, except a very small opening for drainage; the finger placed upon a straight splint, and passive motion adopted in from ten days to a fortnight.

I append the following brief notes of a case in which the result of the above treatment was most encouraging. F. G., a French polisher, came to me in June, 1877, asking me to remove his finger, the first finger of the right hand. On examining the finger I found the whole of it swollen up to about double its natural size, œdematous and dark-coloured. He was unable to move it on account of its condition and the intense pain it gave him, which also prevented him from sleeping at night. There was a sinus by the side of the first phalangeal joint, from which oozed most offensive pus, and a probe passed into the joint struck against bare bone. There appeared to be total disorganisation of the joint, the first and second phalanges rubbing together with a grating feeling, as of two pieces of lump sugar. He had had the finger upon a splint, and it had been treated with lotions, &c. for some time, and his medical attendant had advised him to have it removed. I made a vertical incision, found the heads of both bones completely denuded of cartilage, and removed them, taking care to injure the tendon as little as possible. I then closed the wound, placed a very small piece of twisted carbolised tissue in it as a drainage-tube, dressed it antiseptically, and placed the finger upon a splint. At the end of a week the wound was healed. After about ten days the patient was shown how to use passive motion, and at the end of a month he had a good serviceable finger, which he was able to use almost as well as any of the others.—*Lancet, Aug.* 31, 1878, *p.* 290.

29.—UNUNITED FRACTURE TREATED BY ADDITION OF DOG'S BONE.

By Dr. ALEX. PATTERSON, Surgeon and Lecturer on Clinical Surgery at the Western Infirmary, Glasgow.

Various causes, both constitutional and local, have been assigned for the non-union of fractures. Among the former class, syphilis, scrofula, scurvy, pregnancy, lactation, the presence of cancer, and general debility, are mentioned, whilst paralysis, inflammation of the soft parts, deprivation of blood, disease of the bone, interposition of muscle or tendon between the fragments, the presence of a foreign body, and motion, are given as local causes. Of all the influences referred to, motion, in my opinion, ranks as the chief, if not the only, cause of non-union of fractures.

Very various plans of treatment have been adopted for the purpose of getting the fractured bone to solidify. The method used in the following case is probably novel in this country, but I have heard that a practice somewhat similar has been tried in China, unsuccessfully, however.

D. M., a marine engineer, whilst at sea, on Jan. 3, 1873, sustained a simple fracture of both bones of the left forearm, about an inch and a half above the wrist-joint, caused by his having been driven by a heavy sea against a lifeboat. The arm was put up in splints and kept up for some weeks. On the removal of the apparatus it was found that the bones had not united. He did not reach land for eight months after the accident.

Patient, aged forty-three, and in good health, was admitted to Glasgow Royal Infirmary on Oct. 7th, 1873, nine months after the receipt of injury. Immediately after admission subcutaneous section of the flexible uniting medium was performed by the gentleman in whose wards he lay, and the arm was put up in splints for three weeks, but as union did not seem to be taking place, incisions were made along the radius and ulna, and the bones resected. The arm was again put up, and matters were progressing favourably, when, at the end of four weeks, erysipelas set in, and during its course necrosis of about three-quarters of an inch of the radius occurred. The limb was again put in splints and retained so for six weeks longer, when the external wounds were healed, but the bones had not united. Patient then left the hospital, having been in the house for three months and a half.

Aug. 15th, 1874. The man was readmitted to-day for the purpose of having his arm amputated. In the absence of the regular surgeon I took charge of the case, and, a consultation having been called, amputation was unanimously recommended; at the same time permission was accorded me to make any possible attempt at saving the limb. On examination, the cicatrices of the former operations were seen lying on the inner and outer side of the false joint. The hand and lower fragments were drawn somewhat up towards the elbow, and hung swinging about, completely powerless. The lower end of the longer fragment of the ulna formed a hard, smooth projection, over which the skin was tensely drawn.

After having given the case some consideration, on the 14th of September the patient was taken into the theatre and placed under the influence of chloroform, while at the same time a retriever dog was being anæsthetised. I made an incision along the ulnar side of the arm, cutting down upon the ends of the fractured bone, and removing the fibrous band which alone formed the bond of union; the rounded points were removed

by the saw, and a hole drilled obliquely through each squared end. The same process was repeated on the radial side, when it was found that an interspace of about three-quarters of an inch existed between the two fragments of the radius. In the meantime, Mr. Andrews, one of the senior students, and a very clever manipulator, had exposed the humerus of the quadruped, completely denuded of every tissue except the periosteum. The length of bone was accurately measured (three-quarters of an inch), while from half an inch beyond the end of the necessary length the periosteal covering was rapidly but carefully dissected, the bone sawn through, a hole drilled in either end obliquely, as in the radius and ulna, and at once placed between the ends of the radius, where it fitted accurately. Wires having been passed through the holes, the bones were firmly tied together, the loose half-inch margin of the periosteum of the foreign bone being carefully spread over the periosteum of the radius. The wound was stitched with silver wire, the bone sutures coming out at each end of the incision. Wires were passed through the ulna, tied together, and the wound treated in a similar manner. The entire operation was conducted under the carbolic acid spray. The arm was put up in gauze, and held in two rectangular splints.

Sept. 15th. Patient complains of some pain in arm, but says he had snatches of sleep through the night after having had twenty-five minims of tincture of opium. After the operation there was a slight tendency to sickness, which was relieved by ice. Pulse 80, rather hard; tongue slightly furred, but moist; skin somewhat hot. Dressed; one or two of the stitches removed, as there were signs of tension and a slight blush around the sutures. To have tincture of opium as before. It is needless to give a detailed account of the dressings up till Nov. 3rd, when the ulna was found to be firmly united; but on the radial side small pouting granulations appeared, as if a foreign body were present, which, however, could not be detected by the probe.

Nov. 28. To-day the patient was put under chloroform, and the wires removed. That which had tied together the ulnar fragments was first caught with forceps, and taken out with great ease; it had apparently cut its way through the bone, and was lying immediately beneath the skin. I then went at the radial side with extreme curiosity and anxiety. Over the seat of fracture a small, elongated patch of extremely soft granulations was seated. At one end of the patch, the upper, one wire was caught, and easily extracted; the other was found at the lower end, and a considerable amount of force was necessary for its withdrawal. Although the wire came away complete, it seemed to have been broken in the bone by

the force exerted in extraction. With my finger-nail I scraped off the exuberant granulations, and with a probe examined the wound. Dead bone could not be detected, although the appearance of the small wound led me to suspect its presence. The fracture looked, on the whole, to be fairly united, and the patient was dismissed, with strict orders to return weekly for dressing and examination.

Thirteen weeks have elapsed since the date of operation, and the wires were perfectly bright on removal, the wounds having been kept antiseptic throughout. The man has gained in weight, and improved much in appearance. On leaving hospital boracic lint was used as dressing. The small wound remained open for twelve months, when the dog's bone, reduced to about half its size, came away, after which the wound healed completely. The radius appears to have fallen in somewhat towards the ulna, leaving a slight deformity.

D. M., wearing a leathern support around the forearm, resumed his former occupation, at which he is still engaged. He called on me some weeks ago, and remains in perfect health, and retains a very useful arm. Thinking of Ollier's experiments with the periosteum, of the transplantation of skin from an amputated limb to ulcers, and of the transference of the mucous membrane of the rabbit to the human eye, I had some hope that the strange bone might have found a new home for itself in the human arm; failing which I knew it would secure perfect alignment of, and steadiness in, the ulnar fragments. Should a similar case occur again, I should adopt the same process, still hoping that the two bones might become one.—*Lancet, Oct.* 19, 1878, *p.* 539.

30.—PARAFFIN SPLINTS.

By Dr. Wm. Macewen, Surgeon, Royal Infirmary, Glasgow.

Having for some years past used paraffin for the formation of splints, and having found it possessing considerable advantages, I desire to bring its properties under the notice of surgeons.

At the outset I found that the results obtained by the use of paraffin were variable, uncertain, and untrustworthy. At times a splint might be obtained which was all that could be desired; at others the splint was soft and totally unsuitable. Thinking that this variability might have been due to some impurity in the article, pure specimens were obtained; but this in no way abolished the defects. During these experiments it was found that some specimens melted in five minutes, while others required, when exposed to the same heat, nearly double that time. Again, it was ascertained that those specimens which took longest to melt retained their heat longer, and became

ultimately harder, than the paraffins which melted at a lower temperature. On mentioning these facts, as defects, to the manufacturer, it was found that the first was a well-known quality of paraffin, and that the maker could, in the process of manufacture, produce a paraffin having any desired melting point, from 75° to 150° F. A supply of specimens ranging from 110° to 130° F. was obtained, and a systematic series of experiments were conducted in order to ascertain the most suitable melting point. It was found that the paraffin having a melting point of 110° F. was most agreeable to the patient, but that the splints formed of it were soft, as long as it was kept at the temperature of the body. The high melting point paraffins made the hardest, firmest splints, but their application could not be borne by the patient in consequence of the heat. Under these circumstances one of two courses was necessary: either to discard the paraffin having a high melting point, or to alter the mode of application. At that time the paraffin was merely brushed over the surface of the bandages after they had been applied to the limb, consequently the limb received the heat, and became painful if a paraffin with a melting point of 130° F. was used. Another disadvantage of that method of application was, that the bandages were very apt to receive only a superficial coating of paraffin, which formed a crust over the surface and was apt to crack when cool; this was most likely to occur if cotton bandages were used. After many different plans were tried, it was found that soaking gauze bandages in the paraffin and applying them to the limb as an ordinary roller obviated the difficulty of causing the patient to feel the heat, and at the same time it produced a much firmer and more compact splint than the old method. If a semi-elastic bandage is required, the surgeon should order the manufacturer to forward a paraffin with a low melting point, say 110° F.; bnt if he desire a firm immobile splint, let him order a pareffin with a melting point of 130° F. It should be perfectly pure: those having an odour of naphtha are not so, and consequently ought not to be used. It is the ordinary commercial solid paraffin which is supplied. It is formed in blocks, and is a perfectly odourless, translucent or whitish neutral solid substance. In this state it remains for any length of time without suffering from atmospheric exposure; water or moisture has no effect upon it; in fact, it is almost indestructible except by burning.

When the paraffin is required for use, the first step is to liquefy it. In order to hasten the process of liquefaction, the block should be scraped down with a knife or broken into small pieces with a hammer. It may be liquefied in various ways, but the simplest and most cleanly is to place the paraffin in a vessel which should then be immersed in another containing

boiling water. These two vessels may be placed on a table with something covering them, and, if well managed, the paraffin will be found melted in from ten to fifteen minutes, or even less, The gauze bandages, previously rolled and ready for application, should then be dropped into the melted paraffin and in five minutes they become saturated. They are then lifted, the superabundant paraffin gently pressed from them, placed on a plate, and when sufficiently cool to be handled by the surgeon he takes them up and applies them to the limb. The nurse may have the bandages quite ready for the surgeon, who has then only to apply them to the part. A dry gauze bandage ought first to be applied to the limb, to prevent the paraffin bandage from adhering. If a very firm splint is desired, a coating of hot paraffin may be brushed over the bandage after two or three layers of the roller have been applied to the limb,—just in the same way as one would do in making a starch bandage, in which case he would apply a starched roller first, and then rub a quantity of starch round the limb before applying a second. The splint begins to cool in a few minutes after application; but the cooling may be hastened or retarded at will, by allowing or preventing a free current of air to play over it. In about fifteen minutes to half an hour it will become firm and solid. The bandage spoken of may be made of ordinary coarse gauze, or of any thin loose material which will form a framework for and retain a quantity of the paraffin in its meshes. If the splint be required for a compound fracture, the antiseptic gauze may be used, and in this way an antiseptic, or at least a neutral, splint may be obtained. If windows or apertures be wanted, these may be cut out with a pair of ordinary dresser's scissors immediately after the splint has been adjusted, before it is quite solid. If, for any purpose, a splint is required to be removed from a limb, this may be done while the splint is still warm by a pair of dresser's scissors. Holes may be punctured in the splint and the apparatus readjusted, and afterwards held firmly together by a lace. After the splint has become firm and solid it may be cut with a sharp pocket-knife, though this is not so easy as cutting with scissors while the splint is still soft.

Paraffin splints do not shrink or contract on the limb in the same way as starch and plaster-of-Paris bandages do. This at first sight might seem strange, as every body contracts as it cools, and paraffin forms no exception to this rule. But this apparent contradiction may be explained by bearing in mind that the first part to cool is the external layer, the internal being kept warm both by its own heat and the heat of the body, so that the cooling will take place from without inwards, and the contraction will take place towards the outer layer. As a con-

sequence of this fact, the bandages should be applied with the firmness which it is necessary to maintain. If any doubt remains regarding the amount of pressure exercised, the surgeon will be able in a few minutes to see the result of the pressure on the extremity of the limb.

The hands of the surgeon will get smeared with paraffin, but this can easily be removed by washing them in hot water. Before touching the melted paraffin, if he rubs his hands over with glycerine, the paraffin will not adhere in the same way. A piece of india-rubber sheeting or a newspaper should be placed under the limb to catch any drops of paraffin which may fall. If the paraffin is allowed to cool, it will then scale off from these articles, and very cleanly so from the india-rubber sheeting. If any drops fall on the clothing, they will brush off if allowed to dry, or if the article be placed in hot water the paraffin when melted will rise to the top of the water. All scraps of paraffin may be collected and kept for future use. When the splint is for no further use, it may easily be removed by cutting it off with a sharp knife or a strong pair of scissors. If it be on a limb, it can be immersed in hot water, and when it becomes pliable it may be easily removed. After it has been taken off the limb, the bandage should be plunged into hot water—boiling water,—when the paraffin will rise to the top and float, while the bandages and any dirt which may be on the paraffin will fall to the bottom. The dish should be permitted to stand until the water cools, when the paraffin may be removed in shape of a solid cake. It is thus ready for use a second time. In this way it may be used again and again.

Not only is it of use for ordinary splints, but it may be formed into any conceivable shape. Sayre's ideas regarding spinal bandages may be admirably carried out in paraffin. After the bandage has been applied during suspension, the jacket may be cut up the side, holes punched in it, and the apparatus may then be taken off and put on at pleasure, it being laced like a pair of stays. It may be taken off at night, if that be desirable, and it permits of the performance of the ordinary ablutions which add greatly to the patient's comfort. If in any case it is necessary to make the inside of a splint impervious to moisture, a mould of the limb ought first to be taken by applying the paraffin bandage in the usual way, then cutting it up, and brushing the inside and cut margins with paraffin. Such a splint will then resist the imbibition of discharges, and the wounds in the limb may be syringed out without detriment to the splint.

To summarise: Paraffin, as a material for the formation of splints, possesses the following advantages:—1. It is always ready for use. It may be kept for any length of time without

spoiling or undergoing change, unless it is exposed to heat. 2. It is easy of application, requiring no special training or dexterity, and it may be ready for the surgeon about fifteen minutes after he has ordered it. 3. It sets readily, and yet not too soon. Starch and glue take a much longer time to harden, glass longer still, while plaster-of-Paris sets too quickly for many purposes. 4. Before it sets it may be cut with a pair of scissors and afterwards with a sharp knife. 5. It may be made into a splint quite impervious to discharge or moisture of any kind. 6. It is light, easy, and comfortable to the patient. 7. It may be formed into a semi-elastic bandage, or a firm solid splint. 8. It may be used many times. After one splint is finished the paraffin can be remelted and reapplied.

Regarding its defects, the one usually spoken of is its liability to ignite. This fear is founded on the idea people entertain of the inflammability of paraffin oil; but block paraffin has quite a different character. When pure, all the naphtha has been removed from it. A spark or a lighted match will not set it on fire. If it be thrown into the fire, it will burn like grease, but the splint will not ignite from any ordinary circumstance. There is no likelihood of any atmospheric temperature in this country affecting splints made from paraffin having a melting-point of 130° F.; and for use abroad a paraffin having even a higher melting-point may be obtained. In melting the paraffin, if it be done in the way described, by boiling water, there can be no possibility of it igniting. As paraffin candles—which are not pure paraffin, however,—are obtainable in most villages where pure block paraffin cannot readily be got, experiments were tried with "Young's silver paraffin candles," and they were found to make a firm splint, which might be used in an emergency.

Paraffin would make an excellent addition to the army surgical stores; it is light to carry, it is not easily injured or destroyed, it will float in water without damage, and can be formed into any kind of splint.—*Lancet, Aug.* 31, 1878, *p.* 288.

31.—PLASTER-OF-PARIS SPLINTS FOR FRACTURES OF THE LEG.

By JOHN CROFT, Esq., Surgeon to St. Thomas's Hospital.

For more than two years I have been using at this hospital plaster-of paris splints for all simple and for most compound fractures of the lower extremity. The apparatus consists of—first, inside and outside splints made of common house-flannel and plaster-of-Paris; and, secondly, of muslin bandages. The splints for fractures below the knee are shaped somewhat like the old short outside splint. The foot-piece is, however, wider. The splint for the inside of the leg is similar in length and width

to that for the outside. The splints should be long enough to extend from the tubercle of the tibia to the middle of the metatarsus, and together they should be in width about one inch less than the circumference of the limb at the corresponding part. A rough guide to the shape of the splint may be found in the injured person's stocking when it is laid flat on a table. Each splint is constructed of two layers of the flannel. The outer layer carries the gypsum. The inner layer forms a dry, warm, elastic lining, and protects the skin. These splints are applied by means of the muslin bandages. The bandage is put on like any other, from the toes to the knee. One thickness is enough. Two bandages of five or six yards in length are more convenient than one of ten or twelve yards.

To make the splints.—1. A piecé of house-flannel or an old thin shrunk blanket, or any suitable substitute, is selected. The pieces may be shaped by measurement, taking the circumference of the limb below the knee, at the biggest part of the calf, just above the ankle-joint, from the front of the ankle-joint round the heel to the front again, and at the middle of the metatarsus. The flannel of each splint should be in width half an inch less than half the circumference at any of those points. The width of the two splints should be one inch less than the circumference of the limb at any corresponding part. It should be long enough to extend from the tubercle of the tibia to the middle of the metatarsus. Four pieces are required—two for each splint. 2. Two bandages of common muslin are prepared, each five to six yards long and two inches and a half in width. 3. About a handful of good dry plaster is mixed with water to the consistence of thick cream. 4. The inside pieces of flannel may be laid on the table or bed, the outer surface being upwards. 5. The outside pieces are to be soaked in the plaster separately, and laid out on their respective inside pieces.

Application.—Whilst traction is kept up, and the ends of the broken bones are maintained in apposition, the splints are to be applied and smoothed; then the bandage is to be put on. Traction is to be maintained during the hardening of the plaster. The latter takes place in about three minutes. Next the limb should be laid on a large soft pillow, the toes directed upwards, and the knee a little bent. In the application of the bandage great caution should be observed that it is not drawn tightly anywhere, and that no one turn of the bandage is tighter than another. The support is to be equal everywhere. The two splints should not meet by about half an inch either down the front or back. The intervals are spanned by the dry porous muslin; at the sides the bandage is fixed to the splints by the plaster, which oozes into it from the outer layer of flannel. If it become necessary next day, or later, to ease the splints, or

to inspect the limb at any spot, the bandage can be slit up with scissors along the middle line in front. One or both of the splints can then be eased from the limb, and readjusted by the addition of another bandage. It is undesirable to wholly remove the splints. They are hinged together at the back by the muslin bandage which spans the interval there. The trimming of the apparatus may be done as soon as the plaster shall have hardened. Should the surgeon be short-handed with regard to assistance, he may apply the outside splint first, and lightly bandage that on; and, when that splint has nearly hardened, he may put on the inside one. As swelling subsides, and the splints become more or less loose, an additional bandage should be put on.

At the end of ten days, if the patient is convalescing, the outside bandage may be gummed, or a fresh-gummed bandage rolled on. That apparatus will last until splints are no longer needed. At the end of a fortnight, or three weeks, as the case may be, the patient may leave the hospital for his own home.

This mode of treatment is admirably adapted to oblique fractures, accompanied by displacement of the tibia, to cases of Pott's fracture, and to comminuted fractures.

Immediate use of the apparatus.—The splints are to be put on when the surgeon is first called to the case. Swelling from contusion and subcutaneous laceration, uncomplicated with lesion of the vascular trunks, is not an objection to the immediate application of the apparatus. On the contrary, the support and enforced rest have a beneficial influence in controlling swelling and its consequent pain.

Covering the seat of fracture by bandage and splint.—I had never observed that any benefit had been derived from the old practice of leaving those parts exposed. On the contrary, I had thought that the swelling and vesication were aggravated by the omission to support those parts. I have experienced only good results from covering the parts. If the fracture has not been properly reduced, the compression of the skin by the bone and splint will probably cause a slough. My remarks apply to those cases in which the bones have been properly coapted. In these cases the pain is relieved, and the swelling is modified, by the equable support afforded by the splints and bandages. I have not hesitated to resort to them two and five days after the receipt of the injury, when swelling, and, indeed, vesication, had already occurred. I may particularly refer to two cases of Pott's fracture, in which the dislocation had not been thoroughly reduced. Those cases were dressed a week ago to-day, and are now under observation. In one of them the skin over the inner malleolus was already turning black. In the other the leg was swollen, red, and vesicated. The deformi-

ties were reduced and the fractures were set whilst the patients were under the influence of ether, and the plaster splints were fitted on, to the great comfort and welfare of the sufferers. The limbs had been previously treated on Liston's back splint.

I would here insist on the relief and other advantages which ensue when thoroughly equable light support is afforded to a broken limb. It assists to make and it maintains extension, it prevents the recurrence of dislocation, and it obviates the irregular spasmodic muscular movements which occur to an imperfectly supported broken limb. The old short inside and outside splints do not afford these advantages. The limb is unevenly squeezed between the two unyielding concave pieces of wood, or between one splint and a bandage. Similarly, when the limb is bandaged into Liston's back splint, it suffers compression between the two appliances. These splints are also not well adapted to maintain extension.

Plaster-of-Paris bandages.—These are capable of ensuring all the desiderata, but I do not employ them for the following reasons: first, that an inexperienced bandager may create uneven pressure by drawing one turn of the bandage tighter than another, or by crowding on the bandage and plaster more thickly at one part; secondly, that, if the bandage is to be taken off, the whole thickness of the plaster and bandage must be tediously cut through; thirdly, that the bandage must be reapplied as a whole, and the limb therefore subjected to loss of support, remanipulation, and probably resetting.

The *lateral* splints, hinged together by muslin, present none, or as few as possible, of these risks and disadvantages. The softness and elasticity of the flannel obviate the risk from uneven bandaging, and the span of soft muslin between the front edges of the splints can be easily cut down with ordinary scissors, as I have already pointed out.

The nearest approach to the excellence of these splints is found in what is known as the "Bavarian" splint. It is, however, less easy to maintain efficient extension during the fixation of the latter splint than it is to do so during the same process with regard to the lateral splints; but the more serious objection is in the fact that the "Bavarian" splint must be taken off for the purpose of trimming. That step entails upon the patient remanipulation, perhaps resetting, and its attendant pains.

Starch, water-glass, glue, gummed and other fixed bandages.—These all have the objection that their drying, hardening, or stiffening are slow processes. Plaster of Paris hardens in about three minutes, less or more. Splints of this material possess as much durability as can be required, especially when they have been protected by the addition of a gummed bandage.

When the patient is convalescent, but still needs some support from splints, the side-splints can be taken off and trimmed, eyelet holes can be inserted along the front edges, and the splints can be laid on or removed at will. If the patient be restless, or become the subject of delirium tremens, the fractured limb, secure in its all-but-complete case, may be swung in the ordinary suspensory apparatus, or may even be left free to be jerked about without much, if any harm. Fractures near the knee-joint, and fractures of any part of the shaft of the femur, have been successfully treated with the aid of this apparatus, The pain just above the heel, which so commonly plagues a patient whose leg has been imbedded in a Liston's back splint, is never complained of by those whose fractures are put up in these plaster splints.

Adaptability to country and private practice.—These splints are characterised by their simplicity, stability, and economy, and therefore commend themselves strongly to the country practitioner. Instead of wooden or metallic splints, which may or may not fit, the surgeon can take out with him, to his case, a bag of plaster of Paris and the muslin bandages, and perhaps the flannel. The plaster, which should be good, but need not be the very best, must be dry, and therefore should be kept, when in store, in a dry warm place. House-flannel does not appear to require "shrinking. The surgeon should be cautious in using any flannel which has not been in some way shrunk. Ordinary new flannel might shrink on the limb, and fail to yield to the swelling.—*Lancet, June* 8, 1878, *p.* 819.

32.—ON DISLOCATION OF THE HIP.

By HENRY J. BIGELOW, Prof. of Surgery, Harvard University, Surgeon of the Massachussets General Hospital.

The simplicity of the principle which controls hip-reduction is as yet scarcely appreciated by the majority of practitioners into whose hands the scattered cases fall. Writers also, until very lately, have seemed disposed to consider "manipulation" as but one of several means of reduction of equal value; occasionally available, indeed, but complicated with many methods, and by conflicting opinions regarding its essential features,—by perplexing talk of abduction and inversion, of flexion and partial flexion, of rotation and circumduction. I have, perhaps, myself unintentionally contributed to this erroneous belief. But my paper upon this subject was based upon the analysis of a large number of dissections, experiments, and cases of reduction, which could neither be condensed advantageously nor yet omitted, while its object was to show the relation between hip-dislocation and the ilio-femoral ligament,

then generally unrecognised. The whole matter is really very simple. The word "manipulation" is an unfortunate one. "Flexion" is better. The modern method of hip-reduction, whether by lifting or by mere abduction, is the "flexion method," and it supersedes all others.

A common way of describing dislocation is by the terms backward, forward, in front, and behind. It should be borne in mind that when the body is erect, the pelvis is oblique: the acetabulum standing in like manner obliquely, at an angle of about 45°, facing the front and outside of the thigh, at a point an inch or two below the trochanter; so that what is behind the socket may be either below or outside of it. In fact, it is not easy immediately to place an os innominatum, or even a pelvis, in the normal erect attitude to which alone these terms should refer.

As preliminary to reduction, the patient should be etherised to relaxation, and, in order to give the surgeon control of the limb, laid on the floor. It is well to remember that the head of the femur always faces the same way as the internal condyle.

If there is any single and best rule for reducing a recent dislocation of the hip, it is to get the head of the femur directly below the socket by flexing the thigh at about a right angle, and then to lift or jerk it forcibly up into its place. This rule applies to all dislocations except the pubic, and even to that when secondary from below the socket. I have taught it many years. A case reduced by this method under the care of the distinguished surgeon, Mr. Erichsen, will be found in the Lancet for 1872, vol. i., p. 10. Of the various ways which incorporate the essential principle, this one was placed first in my paper, in connexion with the common dorsal dislocation; and I elsewhere showed how it was applicable to other dislocations. The reduction by the lifting method is usually instantaneous; and flexion is at the basis of its success.

But if, after one or two trials, it should appear that the hip cannot be jerked into place, let the rent in the capsule be enlarged a little by moving the flexed thigh, not up and down, but from one side to the other, so as to sweep the head of the femur across below the socket. No danger need be apprehended from this expedient of circumduction. The added injury is a very slight one. So long as air is not admitted to the wounded parts the lesion is no more serious than often occurs in a simple fracture of the thigh. The laceration which resulted from the old longitudinal traction with pulleys was often much greater, and that from ill-planned and protracted efforts by flexion is always so. Indeed, such additional laceration may sometimes advantageously occur without the knowledge of the surgeon during unsuccessful efforts to reduce the bone, especially in

executing the manœuvre described in the rule, "Flex, abduct, evert."

This familiar rule, until I explained its mechanism, was an empirical one. Flexion is indeed the essence of it; but besides this, the femur is rotated around the ilio-femoral ligament as a centre. When the knee, abducted by this rotation, descends on the outside the head of the femur rises on the inside, like an opposite spoke in the wheel and is thus prized into place by the shaft of the bone as a lever, with the outer band of the ligament as a fulcrum. Eversion is of less importance, but helps the movement by inclining the head of the abducted femur toward the socket. It is best effected by keeping the foot of the flexed limb stationary while the knee is pressed outward. By the great power it gives the operator, this method, or rather the circumduction connected with it, is especially useful in breaking the adhesions of an old dislocation. It might be called the abduction method, to distinguish it from the lifting method.

Abduction sometimes succeeds at once. It does not answer, however, when there is much laceration of the capsule. Then the head of the bone will not rise. Suspended by the ligament which is attached at the trochanters, it slips backward and forward below the socket, from the dorsum to the foramen ovale. The upward lift then becomes absolutely essential. In one of the figures of a standard and very excellent modern English surgical work, which has done me the honour to refer to my views, the engraver has placed the hand of the surgeon above the knee, adding to the weight of the limb, and bearing the head down below the socket, where it might even hook up the sciatic nerve. The weight of the limb should be sustained with the hand in the ham.

Such are the simple principles of hip-reduction, which are still often buried beneath unimportant details, and sometimes under a flood of technical language.

By the lifting method I have reduced, without haste, a dorsal dislocation of both hips in the same man in less than two minutes. With an effort to do it quickly, I reduced in two seconds a dorsal dislocation that had just been vainly "manipulated" under ether, by two medical practitioners, for an hour. A few months ago I forcibly lifted into its place from the dorsum, in a few seconds, a dislocated hip, which required traction with the whole strength of the elbow engaged under the knee, against the foot upon the pelvis.

By the lifting method combined with abduction, I have lately, in the case of a little girl of seven years, reduced a dorsal dislocation of five months' standing, said to have occurred during the delirium of typhoid fever. To keep the bone in place

when reduced, it was necessary to tie the knee to the side of the bedstead, in extreme abduction, after securing the foot to the knee of the sound side, in order to maintain the limb in the final position that reduced it.

Since this occurred I have reduced by simple abduction, and in a moment, a recent dorsal dislocation in a little boy of four.

From these and other cases may be gathered the following rules for reduction:—

In the *dorsal* dislocation—

1. *Flex and forcibly lift.* If this fails,—

2. *Flex, and lift while abducting.* If this fails, it will be found that abduction has carried the head of the bone from the dorsum nearly or quite to the thyroid foramen, and that the capsular rent has been so enlarged that the first method may now prove successful. Lifting the femur abducts it if it raises the pelvis on the lifted side. In *thyroid* dislocation, *adduction* of the flexed thigh reverses this movement and carries the head from the thyroid foramen to the dorsum, also enlarging the opening and making the first rule effective. The *pubic* dislocations may be generally brought down, after flexion, without difficulty from above the socket. If they are secondary, the head of the bone will fall, after flexion, to its previous position below the socket, and may be reduced from there like the thyroid.

My belief has long been expressed that the flexion method is the only rational one, and that pulleys are practically obsolete, unless, perhaps, to steady the limb, in some rare case, by rectangular not longitudinal traction. Then alone is the tripod apparatus possibly useful. But why flex the thigh? Because it relaxes a part of the ilio-femoral ligament. When the limb is straight, this ligament is rigid, and the bone fixed. When it is flexed, the inner band is slackened—as also the whole ligament if the limb be lifted—and the head is brought down below the socket, level with the capsular rent, becoming movable for reduction, as it was previously for escape.

Now flexion, with adduction or abduction, is the habitual attitude of the thigh, especially in action or for self-defence; and the ligament is thus habitually relaxed. On this account, and also because the capsule is weak and thin below, "like wet bladder," and the socket margin notched on that side, the dislocation downward is the most common one. All this I have elsewhere shown, and also that the head of the bone, thus escaping primarily below the socket, generally at once slides up to a second position on one side of it or the other. In fact, downward dislocation is so frequent, and the route from below to the dorsum is rendered so easy both by muscular contraction and by the conformation of the bone, by the latter especially,

that in a very large proportion of cases the displacement is found to be dorsal. The result has been to extend the application of any rule for reduction which applies to dorsal dislocation, and to make it comparatively familiar.

These secondary dislocations from below the socket are well known; and all the regular dislocations (if we except those between the small rotator muscles) may be secondary. On the other hand, there can be no question that the bone may be primarily dislocated in various directions. Flexion does not in all dislocations return the limb to a point below the socket without increased laceration of the capsule, as it would if they had all come from there. Just as the downward dislocation may take place during extreme flexion, especially with rotation inward, and through a comparatively small aperture, so the dislocation on the pubes may occur during extreme extension; and it is usually reduced from above the socket. A direct thrust backward may produce dislocation upon the dorsum when the ilio-femoral ligament is relaxed. Primary dislocations are not rare, in various attitudes of the limb.

But in order to compare the regular dislocations more readily, let us for the time consider them all as secondary from a common point below the socket, and follow the progress of the bone from the moment of its original escape downward to this point.

The femur is, then, sometimes held below the socket in extreme flexion, firmly, as if the neck were embraced in the capsular rent. Such dislocations have been considered anomalous, and described as "upon the perineum," and "upon the tuberosity." I have shown them to be regular. But it oftener happens that from after-violence, or by its own weight, the knee falls, the thigh is straightened, and the head of the bone, suspended at the trochanters, is prised upward on one side or the other of the socket, lacerating the capsule. In this view, the downward dislocation is a first stage of the others, the head pausing below the socket and hesitating which side to go: whether to the thyroid foramen, or above it to the pubic region, or even to a higher point, where the dislocation is still a regular one (the subspinous), all these being internal to the socket; or, which is more common, external to and behind the socket on the dorsum, where, as I have shown, the limb is at once inverted by the outer band of the ilio-femoral ligament. The first serious obstacle the bone then encounters in its dorsal ascent is the strong obturator internus muscle and the subjacent capsule. This is one of the more common dislocations. If the bone ruptures the obturator and the capsule, it rises to the pyriformis, and, if this be broken, to the gluteus minimus, retaining the usual features of a dorsal displacement. (See an an account of the autopsy in Todd's case, No. 40 of Cooper.)

If, however, the outer band of the ligament be ruptured, the limb is no longer necessarily inverted, but capable of eversion, and the dislocation is then the everted dorsal. The head of the bone, still suspended by the remaining inner band, can now be hooked over it above the spinous process, the dislocation is then supra-spinous, the limb being still straight. But if the femur has been hooked over the entire ligament, instead of the inner band only, the limb cannot hang straight; it assumes a very oblique attitude across the axis of the body. This I have called the anterior oblique dislocation. These three positions of the bone were described by Sir Astley Cooper as anomalous; but the explanation here given of their essential mechanism shows that they occur under prescribed conditions of the ilio-femoral ligament, and that they are therefore constant and regular. In short, all regular dislocations, including those upon the perineum and the tuberosity, as well as the everted dorsal, the anterior oblique, and the supra-spinous varieties, have constant and distinctive signs which they owe to the ilio-femoral ligament; and eversion in dorsal dislocation signifies that the outer band of this ligament has been severed.

Nothing can be more simple than the reduction of secondary dislocations from below the socket. If dorsal, and the flexed thigh has fallen to a horizontal position by a spiral movement downward and inward, reverse the movement and reduce the limb by a spiral upward and outward. If thyroid or pubic, and the knee has followed a spiral downward and outward, reduce it by a similar movement upward and inward. But the spirals of reduction practically amount to mere flexion; therefore, in either case, flex the limb and jerk it upward; and in the thyroid luxations it is well to aid the process by outward traction with a towel or by a fulcrum in the groin. Finally, if the capsule offer resistance, sweep the flexed femur from side to side to separate its fibres and get the head below the socket, before jerking the bone up into place.

There is another point, which is now of less practical importance. By the now obsolete straight traction with pulleys reduction from the dorsum was sometimes difficult, or failed. Surgeons then called the dislocation "ischiatic." There was a superstition about the ischiatic notch. It was the maelstrom of the hip-bone. There was obviously in those days some real difficulty, which does not occur with the flexion method. Its most frequent source was the strong obturator internus muscle combined with an unusually sound and tense falciform edge of the subjacent capsule, interposed between the head of the bone and the socket, occluding the latter.

On this account I substituted for the term "ischiatic" the phrase "below the tendon," as explanatory of the manner of

the displacement, the relations of the bone, and the general character of the difficulty of its reduction. The phrase "*below* the tendon" was desirable to distinguish this dislocation from those in which the head emerges *above* the tendon. In either case the head of the bone may reach and occupy a place "behind" the tendon.

When my paper was written, the bugbear of the ischiatic notch was still feared, because the practice of reduction by straight extension was still in vogue. In fact, surgical works are still illustrated with pulleys from Sir Astley Cooper. For more than fifteen years I have neither employed nor taught reduction by straight extension, and I have long abandoned any especial description of the so-called ischiatic dislocation, whose chief and distinctive sign was that it resisted straight extension. For practical purposes this dislocation is dorsal, and may be called so. It is easily reduced by flexion.

So, also, it is useless to distinguish from the common dorsal dislocation those which lie between the small rotators; because, although interesting to the anatomist and the pathologist, it is very doubtful whether they will ever be diagnosticated with certainty during life. They exhibit, as I have shown, more inversion, more immobility, and more elasticity; but these signs are not pathognomonic. When these dislocations occur, the head of the bone is thrust directly backward through the capsule, and escapes, this time, not below the tendon of the strong internal obturator, but above it, by the muscular interstice between this muscle and the pyriformis, or even higher, between the pyriformis and the gluteus minimus. The bone may possibly be reduced from these positions by vertical traction, or prised back through the interstice by the abduction method or by outward rotation; but I think such attempts will generally end in rupturing the muscles. The dislocation would then offer no distinctive features, and would belong to the common dorsal variety, in which we can only guess what rotators are ruptured. Indeed, it is the rule in dorsal dislocation that the quadratus or some of the rotators are torn; which of them we need not know. Our business is to reduce the bone by flexion,—and by circumduction also, if that be necessary to rupture the capsule and muscles, and so to clear the way to a point below the socket, from which the femur can be lifted into place.

If, in accordance with these views, we suppress any distinct mention of the inter-rotator dislocations,—which indeed have never been separately classified (although, if it were possible always to identify them, they would deserve to be separately considered)—and of the obsolete ischiatic dislocation, by whatever name we call it,—we have the following classification of regular dislocations, based on their direction from the socket.

External to the socket,—the dorsal, and the everted dorsal.

Internal to the socket,—on the perineum, the thyroid foramen, and the pubes.

Below the socket,—the dislocation toward the tuberosity.

Above the socket,—the subspinous, the supraspinous, and the anterior oblique dislocations.

Of these, by far the more common are the *dorsal*, the *thyroid*, and the *pubic*, which offer the most convenient division for practical purposes. It leads to the following practical grouping of dislocations, based on the usual mechanism of their occurrence and of their reduction.

Dorsal,—the dislocation on the tuberosity, the dorsal, the everted dorsal, the anterior oblique, and the supraspinous.

Thyroid,—that on the perineum, and on the thyroid foramen.

Pubic,—the pubic and the subspinous.

The ilio-femoral ligament is unbroken in these regular dislocations,—excepting only the everted dorsal, and supraspinous varieties, in which its outer band is severed, while the inner band remains. All may be readily reduced after flexion, with or without circumduction: the thyroid and pubic with a towel or fulcrum in the groin, if need be—in the one for outward, in the other for downward and outward traction. This method covers the reduction of the primary as well as the secondary forms of luxation.

It may here be added that a broken socket makes reduction difficult or impossible,—and that if the femur tends to escape after being reduced, the flexed thigh should be abducted down to the bed, and confined there at right angles with the body, to take advantage of the tense ilio-femoral ligament in holding the bone in place, as in an instance before mentioned.

Upon the ilio-femoral ligament I have based my theory of hip dislocation, its classification, its mechanism, and its reduction. Other bands of fascia, of capsule, and of muscle may indeed be incidentally concerned in confining the neck of the femur; but their action is a changing one, and secondary in importance. Divide the ilio-femoral ligament, leaving the rest of the tissues, and the mainstay of the characteristic deformity is gone. It is not difficult, indeed, still to work the limb into the semblance of a regular dislocation; but such displacements are exceptional, and their attitude is not constant. In short, they are irregular.

All that is most essential about hip dislocation and reduction may be learned from a pelvis and thigh-bone from which everything has been removed but the ilio-femoral ligament, with perhaps the obturator internus muscle and its subjacent capsule, although the two latter can be spared.

The dissected capsule is a combination of membrane with

fasciculi, beautifully adjusted for universal motion, limited in all directions. The vessel-bearing and so-called "ligamentum" teres within it is never strong, and is sometimes wanting; it is therefore unimportant in dislocation. Of the capsule itself the posterior part combines strength with great mobility, its pelvic insertion being far stronger than its femoral insertion. A general notion of the arrangement of the posterior part of the capsule may be had if we suppose that it is extended from the socket half-way to the femur, being there reinforced by a thickened edge and tied to the trochanters by bands. The thick edge is represented by a fasciculus which is said to be "annular." It is parallel with the rim of the acetabulum, and midway of the capsule. It aids the pelvic half of the capsule in its effort to retain the head of the femur, and, in the extended limb, tightens it. It is tied to the trochanters by the ischio- and pubo- (or pectineo-) femoral bands, which, as they pass from the pelvis to the trochanters, cross the annular ligament and are incorporated with it. In these cross-bands lies the main strength of the femoral insertion of the posterior capsule. Between them, at a point between the trochanters, is its weakest part—a mere membrane, supported by these fasciculi. It is often accidentally opened in dissection; and when torn along its margin, as sometimes happens in dislocation, after the bands are ruptured, the wide flap may occlude the socket as effectually as in Fenner's case.

But among these minor anatomical details connected with the posterior capsule, it is important not to lose sight of the main surgical facts. To these facts the ilio-femoral ligament is the key. It lies mostly in front. In an anterior view it is triangular, narrowest above. Its inner part, the ligament of Bertin, limits extension of the limb; its outer part limits eversion; while to the latter alone belongs the inversion of dorsal dislocation. The functions of its outer and inner portions are therefore largely distinct. The habitual action of the thigh tends to develop them separately. In fact, they can easily be distinguished by the direction of their fibres, some of the fibres of one branch being inserted into the other half way up. They are separated by a cribriform interval for vessels, corresponding to a frænum of soft capsule inside the joint, which adds to the thickness, but little to the strength of the ligament. They vary in development in different subjects; the outer band is not unfrequently the more voluminous, and, as is stated in my paper, whole ligament is sometimes of uniform thickness. In that case its margins act as bands.

In recent times the narrow ligament of Bertin has been generally described and recognised as the ilio-femoral ligament. It is but the inner part of it. Its outer band, and even its tri-

angular shape, were scarcely known to modern anatomists, and were unknown in their surgical application to dislocation when I came across them in dissection. I afterwards found that while the Webers describe the ligament as simply triangular, the anatomist Winslow, and especially Weitbrecht, still perhaps the highest authority on ligaments, had described the "binæ divaricationes" a century and more ago. Thus much for the lower and sometimes distinctly forked insertion of the ilio-femoral ligament along the anterior intertrochanteric line of the femur.

The upper or pelvic insertion of this ligament is into the front of the inferior spinous process of the ilium, and also into the outside of this process, along a rough depression existing beneath the reflected tendon of the rectus, three-quarters of an inch or more in length. All this part is thick and arrests displacement directly upward. From this pelvic insertion, the outer margin of the outer band runs to the trochanter major, and the inner margin of the inner band toward the trochanter minor. Together the two bands constitute the strongest ligament in the body. While it is not difficult by circumduction to tear the whole capsule on either side up to the margin of the bands, these resist. In a strictly surgical point of view, the exact extent of their varying interval may have little importance. But in order to emphasize their separate normal functions, and especially the fact that the characteristic attitudes, the mechanism, and the reduction of hip dislocation are essentially dependent sometimes upon one of these sets of fibres and sometimes upon the other, I have given to the whole, as brief and suggestive of its "binæ divaricationes," the name (inverted) Y ligament. — *Lancet*, *June* 15, 22, 29, 1878, *pp.* 860, 894, 930.

33.—CONTRACTION OF THE FINGERS (DUPUYTREN'S CONTRACTION),

AND ITS SUCCESSFUL TREATMENT BY SUBCUTANEOUS DIVISION OF THE PALMAR FASCIA, AND IMMEDIATE EXTENSION.

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Contraction of the fingers takes place from a variety of causes; and the pathological conditions will be found to vary according to the nature of the producing cause, such, for example, as local injuries, with laceration of tendons, deep abscesses in the palm of the hand, burn-cicatrices, &c. But in the present paper I propose to direct attention to one form only of finger-contraction, commonly met with in men about the middle or beyond the middle period of life—very rarely at younger ages. I have never seen it in women.

The contraction may affect one finger alone; in which case, according to my observation, the ring-finger is most frequently drawn down towards the palm of the hand, but some authors state that the contraction most frequently commences in the little finger. The contraction having commenced in one finger, however, the adjacent fingers are in most cases gradually drawn down, though to a less extent. I have frequently seen the ring-finger severely contracted, so that its tip would nearly touch the palm of the hand, and the middle and little fingers contracted to about half this extent. I have seen the contraction sometimes limited to the little finger. The index-finger and the thumb usually escape. I have never seen them more than slightly contracted. In these contracted fingers, the articulations are generally in a healthy condition; the joints can be flexed freely, but any attempt at extension is painful, from the resistance offered in severe cases by a tense contracted cord passing from the finger into the palm of the hand, to which the skin of the palm is closely adherent. The skin in the palm of the hand near to the base of the contracted fingers is commonly drawn into thick knotty folds.

The pathology and treatment of this form of finger-contraction always has been and still is the subject of much difference of opinion; and Dupuytren appears to have been the first surgeon to investigate the anatomical conditions by dissection. In the report of his clinical lectures, it is stated that when Dupuytren heard of the death of a man who was the subject of this affection, he "was determined that this remarkable disease should not remain unknown. The arm was given to him, and he made a careful dissection. The skin having been removed from the whole extent of the palm of the hand and the palmar face of the fingers, the fold or the puckering of this structure entirely disappeared; it was, therefore, very evident that the arrangement which presented during the disease did not depend on this cause, but was communicated to it; but how, and by what? The dissection being continued, the Professor discovered the palmar aponeurosis extended, retracted, and diminished in length, its inferior part being divided into cords, which passed on to the sides of the affected finger. In extending the finger, he observed that the aponeurosis underwent a kind of tension of crispation—this was a ray of light; so he considered the aponeurosis was something in the effects [to be the cause, W. A.] of the disease. He cut the prolongations on the sides of the fingers, and immediately the contractions ceased, the fingers returned to a state of semiflexion, and by slight force to complete extension. The tendons were natural; the sheaths were not open; the articulations, ligaments, synovial membranes, and bones, were in their normal or natural state." Since

Dupuytren made this important contribution to our knowledge of this affection, it has been sometimes spoken of "Dupuytren's finger-contraction:" a title as useful as it is also a just compliment to the great surgeon, distinguishing it from all other forms of finger-contraction.

The *commencement* of this form of contraction is sometimes to be traced in one hand, in patients in whom the contraction exists in a severe form in the other, and I have watched its commencement from a flattened nodular induration in the palmar fascia in or just above the transverse crease in the palm of the hand corresponding to the metacarpo-phalangeal articulations. In the beginning, the skin is not adherent to this flattened induration; but in one case now under my observation, the commencement of a fascial-cord contraction can be traced, leading from the transverse crease to the little finger in the left hand: a spot certainly not exposed to pressure.

In the *second stage*, the skin becomes adherent to the thickened fascia, and a puckered dimple is produced, extending from which towards the finger a thickened band of fascia can be distinctly felt.

The *third stage* consists of an increased thickening of the palmar fascia and the band leading to the finger; and also the formation of a thick cord-like band of fascia, leading from the central thickening towards the annular ligament of the wrist, accompanied with a gradually increasing drawing down of the finger or fingers towards the palm of the hand. Such I believe to be the gradual progress of the fascial contraction and its adhesion to the skin in Dupuytren's finger-contraction.

Cause.—With regard to the cause of this affection, most authorities agree in assigning it, in the great majority of cases, to a local cause, and believe it to be produced by pressure from the use of tools in various occupations; and it is said that carpenters, gardeners, and gunners, are specially liable to it. Sir James Paget refers to the elder men occupied in wire-drawing and lock and key-making, as being subject to this condition. It is also generally admitted that this form of contraction may take place from constitutional causes, independently of any local cause, and that it is then traceable to a gouty diathesis. Sir James Paget points out the dependence of this contraction in some cases upon the gouty diathesis, and believes that the adhesion of the palmar fascia to the adjacent sheaths of tendons and the integuments forms a point of diagnosis. My own opinion is that it nearly always depends upon a constitutional rather than on any local cause, and essentially I regard it as depending upon a gouty diathesis. In favour of this opinion, I would refer, first, to the class of patients in whom it occurs. During a connection of more than twenty years with the Royal

Orthopædic Hospital, I have seen but very few cases of Dupuytren's contraction in the labouring class; and the cases that did present themselves generally occurred in butlers and in-door servants. It seems, however, to be an affection of common occurrence in the middle and upper classes of society. The cases which have fallen under my observation have occurred in clergymen, barristers, medical men, officers of the army and navy, and merchants; the only condition common to the whole series being a disposition to gout. Co-existing with the finger-contraction, in the cases which I have seen there have been generally other manifestations of a gouty tendency, more especially to that form we recognise as rheumatic gout affecting several articulations and often causing enlargement of the joints of the fingers rather than true inflammatory gout, affecting the great toe. Secondly, I would refer to the frequent occurrence of this affection in the left hand only, and to its occurrence in both hands, which we can hardly explain by any local cause. Thirdly, I would refer to the fact that in several instances I have known two brothers suffer from it; and in some cases, the father and son have been similarly affected; so that, for these reasons, I am disposed to attach far greater importance to the constitutional than to any local cause.

Treatment.—With regard to treatment, this must be either mechanical or operative; and there can be no doubt that gradual mechanical extension, by an apparatus worn night and day, or in slight cases only at night, would not only prevent increase of the contraction, but diminish, and possibly even cure it when slight and not of long duration. But in severe cases, and in those of long standing, mechanical treatment is useless, the only prospect either of benefit or cure being from operative treatment either by open wound or the subcutaneous method.

Dupuytren, in the year 1831, divided the contracted palmar fascia by open wound, making an incision ten lines in length; but a second and even a third incision was found to be necessary. Dry charpie was applied, and the fingers extended. Inflammation and suppuration followed; and at the end of nearly two months, it is reported that "the fingers regained their natural condition." Two other successful cases are also reported in the same lecture.

Mr. South, in his translation of Chelius, states that "Goyrand does not divide the skin transversely, as Dupuytren does, because in straightening the finger, the cut in the skin gapes too much; but he cuts through it longitudinally and through the bridge transversely."

Dr. Otto W. Madelung has recently published an account of the treatment of these cases adopted in the Surgical Hospital

at Bonn by Professor Busch. This operation belongs to the class of open wounds, and consists in dissecting up a triangular flap of skin from the contracted cord in the palm of the hand, and then dividing all the bands of the contracted fascia which can be reached; as the flap thus raised contracts, the lower points of the wound may be united by sutures. "A light bandage closes the wound; the hand is then kept by the patient in a sling, without the slightest attempt to preserve the extension. . . . . Extension-movements with the fingers are only made when the wound has entirely granulated, and then only in a light gentle way. At first, wood cylinders of various sizes are laid in the hand; later, the hand is stretched on a back splint. Active and passive movements are now to be made in, and during the time the hand-bath is used for cleansing the wound. More complicated apparatus than the above are never required. The healing of the wound, accelerated perhaps by skin-grafting, is accomplished in three or four weeks." Successful cases under treatment are referred to, but one is added by the author in which, notwithstanding the use of Lister's antiseptic treatment, suppuration with sloughing of the flexor tendon of the little finger took place; but this is stated to be the only unfavourable case.

Professor Alfred C. Post, of New York, also operates upon these cases by open wound, making incisions at a number of points, but not longer than absolutely required, as he believes that the adhesion to the skin prevents a strictly subcutaneous section from being made. He advocates immediate extension on a metal splint; the dressings to be removed every two or three days, and passive motion applied. Of the four successful cases cited by Professor Post, only one seems to have been of Dupuytren's contraction, the others depending on abscesses and traumatic inflammation.

All operations by open wound for these cases should be condemned as unnecessarily severe, involving a long and tedious reparative process, with the risk of suppurative inflammation, and also a liability to failure, in which event the condition of the patient would be worse than before the operation; contraction from cicatrix being one of the most difficult conditions to relieve. Nevertheless, in England, the operation by open wound has long been practised by the majority of surgeons, and is still recommended in some of the text-books, when the subcutaneous operation has failed, as it is generally supposed to do. There is, however, in England, a very general distrust in all operative procedures in the treatment of this affection, which is regarded as incurable, the patients being advised to bear the ills they have, rather than run any risk from suppurative inflammation, with the doubtful gain of a stiff and useless finger.

The operation which I now propose to bring before the notice of the profession is not based upon any new principle in surgery, as the subcutaneous division of the palmar fascia for contracted fingers has been practised, both in this country and on the continent, for many years. Within the last few years, I have materially modified the details, both with regard to the mode of performing the operation and the after-treatment. The success has been found much greater, and the treatment much less irksome to the patient, as well as of shorter duration, than the method of operating and the after-treatment which I adopted in the earlier part of my practice from the teaching of my late colleagues and predecessors at the Orthopædic Hospital, Mr. Tamplin and Mr. Lonsdale. They believed that both fascia and tendon were involved in the contraction, and divided the contracted cords in the palm of the hand in the same way as they divided tendons: subcutaneously, cutting from below upwards and making only one puncture. They also adopted the same method of after-treatment, by gradual mechanical extension; using complicated steel instruments, with cog-wheels opposite the joints of the fingers. Further study of these cases, and the opportunity which was accidentally afforded me of seeing that the tendons were not involved in a severe case of finger-contraction previously referred to, in which the palm of the hand was torn across by a horse, confirmed me in the belief that the tendons were not implicated in this form of contraction of the fingers, and that surgically we had to deal with contraction of the palmar fascia alone, as Dupuytren had described the affection. I therefore commenced the practice of making multiple subcutaneous divisions of the fascia, introducing the smallest tenotomy knife—smaller than any ordinarily used, and carrying it between the skin and the contracted cord, which I then divide by cutting downwards very slowly and cautiously, taking care not to dip the point or divide any structures, except the contracted band of fascia. In one case, where three fingers were contracted, I made as many as nine punctures and subcutaneous divisions; but where so much is required to be done, I think it generally advisable to confine the operation to one or two fingers, making not more than five or six punctures—in many cases, four will be found sufficient—and leave the other finger or fingers to be treated at a subsequent operation. As a rule I make the *first puncture* at the greatest distance from the finger, in the palm of the hand, towards the annular ligament, a little removed from the point where the skin is adherent to the fascia, and where the skin is not tightly stretched over the contracted cord, so that the tenotomy-knife can be readily introduced between the two. The *second puncture* should divide the same cord as the first, but as near to the

finger as possible, thus leaving the contracted band in the palm of the hand, when adherent to the skin, isolated and cut off from its connections at its upper and lower extremities. The *third and fourth punctures* divide the lateral bands or digital prolongations of the palmar fascia, which usually pass from the central cord in the palm to the adjacent sides of the fingers. These must be divided very carefully in order to avoid cutting the vessels and nerves along the sides of the fingers; the punctures should be made at the bifurcation of the cutaneous net between the fingers, and the incisions directed obliquely upwards and outwards towards the palm of the hand. These incisions will divide the strongest and most prominent bands, which produce the flexion of the first phalanx of the finger upon the hand, and if care be taken to avoid dipping the point of the knife, there will be no fear of wounding vessels or nerves. Sometimes lateral bands of contracted fascia require to be divided opposite the centre of the first phalanx, and this must be done by puncture at the edge of the contracted bands, the knife being directed transversely towards the bone; but this cut must be made very carefully, to avoid the artery and nerve; the surgeon remembering that the band, though tough and strong, is at the same time very thin. These bands are more readily cut through with the scissor-pointed tenotome, *i.e.*, with the point at the extremity of the straight cutting edge. The central-pointed tenotome generally used does not divide the fascial bands so readily or with the same precision. Occasionally a lateral band may have to be divided between the first and second phalanges, at a point corresponding to the articulation, and this must be done very carefully and with the precautions just described. I always avoid making any central incisions in front of either the first or second phalanx, as the sheath of the tendons, or the tendons themselves, may be readily divided by such incisions, and would lead to a loss of the power of flexing the finger after the operation.

Subsequently, I also adopted another important modification in the after-treatment of these cases; viz., the plan of immediate extension, bringing the finger or fingers as nearly as possible into the fully extended position at the time of the operation, and afterwards applying as a retentive apparatus a well-padded metal splint bent to the shape of the wrist, hand, and fingers, and applied along the palmar surface; the finger or fingers, as well as the hand and wrist, being bandaged to the splint. This was a great change from the prolonged after-treatment required by the method of gradual mechanical extension made with the complicated steel instrument with cog-wheels.

I was led to make this alteration from the gradual to the immediate mechanical extension after considering the patho-

logical conditions, and the objects to be accomplished with regard to the reparative process in fascia, as distinguished from the reparative process in tendon.

The object of immediate extension is to widen as quickly as possible the gaps made by the incisions in the fascia, with the view of preventing union of the divided fascia; or to have the union as feeble as possible; objects precisely the reverse of those we desire to obtain after the subcutaneous division of tendons.

Since I have adopted these modifications in the operation and after-treatment, I have had good reason to be satisfied with the results, and in the great majority of the cases operated upon, the contraction has been completely and permanently removed.

Anæsthetics.—With regard to the employment of anæsthetics in these operations, which must necessarily be done slowly and very carefully, several punctures being made, I have always advised the use of chloroform or ether. I abandoned the use of ether-spray, after employing it some years ago, because it was found to harden the skin, so as to make the puncture difficult and obscure the anatomical relations of the parts. It occurred to me, however, that if I rapidly thawed the frozen skin by rubbing it with my own hand, the deep anæsthesia would probably remain long enough for a subcutaneous operation; and this I have found to occur. In the case of a medical friend, Mr. R., upon whom I operated for contracted fingers in January, 1877, and who objected to take either chloroform or ether from the condition of his circulation, I made four punctures, and he only felt one of them, and that very slightly. The objection of hardening the skin, therefore, being done away with, the local anæsthesia by the ether spray is now found to be applicable to tenotomy and some other subcutaneous operations, as its inventor, Dr. Richardson, originally thought it would be.

Summary.—The operation and treatment which I now practise may be described as follows.

1. The subcutaneous division of all the contracted bands of fascia which can be felt; the bands to be divided by several punctures, with the smallest tenotomy-knife passed under the skin and cutting from above downwards: a pledget of lint being at once placed over each puncture and retained in position by a strip of plaster.

2. Immediate extension to the full extent required for the complete straightening of the fingers, where this is possible, and the application of a retentive well-padded metal splint from the wrist along the palm of the hand and the fingers; the fingers and hand to be bandaged to the splint.

3. The bandage not to be removed until the fourth day, when the lint and plaster may also be taken off, as the cutaneous

punctures are always found to be healed by the fourth day. The retentive metal splint to be reapplied, and the hand and fingers bandaged to it.

4. Extension to be kept up by the splint worn continuously night and day for two or three weeks; but the splint and bandage to be changed every two or three days. After this, the extension splint is to be worn at night only, for an additional three or four weeks, free motion being encouraged during the day.

Relapse of the deformity, I believe, is now guarded against by the plan of dividing all the contracted bands of fascia by as many punctures as may be necessary; and, in severe cases, not attempting too much at one operation; and also by the adoption of the plan of immediate extension. If partial relapse should occur—and I have never known more than this—any bands that may have escaped division, or any which may since have become prominent by the extension, may be divided subcutaneously, and the disposition to recontraction prevented. This contrasts very favourably with the relapsed cases after open wound, which from the nature of the cicatricial contraction are incapable of further relief.

It has been my object in the present paper to give to the profession an increased confidence in the subcutaneous operation for Dupuytren's contraction of the fingers, and to point out a number of details in the mode of performing the operation and in the after-treatment, upon which I believe much of the success will be found to depend.—*British Medical Journal, June* 29, 1878, *p.* 928.

34.—ANTISEPTIC SURGERY.

By T. SPENCER WELLS, Esq., F.R.C.S., Consulting Surgeon to the Samaritan Hospital.

Fourteen years ago I called the attention of the British Medical Association, at the Cambridge meeting in 1864, to the causes of excessive mortality after surgical operations. I showed that even where a patient was supplied abundantly with fresh air, yet with the air unsuspected sources of danger might enter through the wound. I directed special attention to the demonstration by Pasteur of the presence of infective germs or organisms in the atmosphere; to the result of the examination by Dr. Angus Smith of the air of large cities and crowded rooms; and to the researches of Chalnet into the varieties of putrescible organic matter, collected from the walls, sashes of windows, and bed-curtains of hospital wards, and in the linen returned from the laundry; and I concluded by an attempt to apply the lessons taught by Polli, who had proved that the development of many low forms of animal and vegetable life was checked or stopped by the use of sulphur and the sulphites.

I think the work of Polli and the use of sulphurous acid, and of the alkaline and earthy sulphites, has been too much disregarded since carbolic acid has been brought into such general use. Polli showed very clearly that whether pyæmia and affections characterised by multiple abscesses followed the introduction of pus into the blood, or symptoms of septicæmia followed putrid infection, or some specific disease followed the inoculation of the specific exudation material; in any or all of these cases, the poison is rendered inactive or neutralised, just as all fermentations of organic matters or the putrefactive changes in animal tissues or liquids are stopped by the action of sulphurous acid. This lecture was reprinted last year, and published by Messrs. J. and A. Churchill. It was two years afterwards that carbolic acid began to be used by Lister in Glasgow. In 1869, at Leeds, it was ridiculed by the surgical orator as a professional error; and it was not till after the address of Lister at Plymouth, in 1871, scarcely eight years ago, that the antiseptic system can be said to have spread widely, either at home or abroad.

With regard to the spray, I have had very great doubt myself whether it is an essential or useful part of the antiseptic treatment. It is exceedingly troublesome, and I think I have seen the cold or chilling of the abdominal cavity injurious. Undue confidence in antiseptics may lead to danger rather than safety, and so far I see no reason for altering the opinion I publicly expressed at Manchester last year, that "for my part I would rather operate in a clean, quiet, well-warmed, and well-ventilated building, be it large or small, without any antiseptic precautions, than run the risk of trusting to the neutralising or destructive power of chlorine or iodine, sulphur or tar, borax, or the permanganates, salicylic or any other acid, in a place tainted by the presence of sewer-gas, or the seeds of some infectious or contagious disease." While ready and anxious to adopt any improvement, neglecting nothing likely to add to the safety of the patients who are confided to our care, we must not be led away by overweening confidence in any antiseptics to omit any of those precautions which experience has proved to be necessary, and we must be quite as cautious against the introduction of any contagious disease as we were before endeavouring to ascertain whether antiseptics will give us additional security. In a word, let us regard the antiseptic treatment not as a substitute for those measures which have already proved effectual, but as an additional safeguard. Time and further experience alone can show what the value of this hoped-for additional security may be. That it will prove to be of real and very great value I hope and believe. Experience, daily accumulating, encourages the hope and strengthens the belief. —*Medical Times and Gazette, July* 20, 1878, *p.* 64.

35.—ILLUSTRATIONS OF ANTISEPTIC TREATMENT IN MINOR SURGERY.

By Dr. J. C. OGILVIE WILL, Surgeon to and Lecturer on Clinical Surgery, at the Aberdeen Royal Infirmary.

The following cases are examples of drainage by catgut introduced by Mr. Chiene, of Edinburgh—a subject of great interest to all surgeons, and of infinite importance to those practising antiseptic surgery.

On January 3rd, 1877, I examined E. McL., aged twenty-three, who had been admitted into the Aberdeen Infirmary in December, 1876, complaining of severe pain in his chest. A swelling, measuring about five inches in length by three in breadth, was found to the left of the sternum above the nipple. Fluctuation was indistinct, but on introducing an aseptic exploratory needle the presence of pus was demonstrated. An incision of sufficient size to admit a forefinger was made under spray, and about two ounces of pus was evacuated. On examination, the ribs were found to be sound. A probe, armed with ten threads of medium catgut, was passed through the opening, and brought out at the further extremity of the abscess cavity, a small slit being made to allow of its emergence. The probe was then detached, and the ends of the drain were left hanging out at either end. Antiseptic dressings were applied in the usual manner, an elastic bandage being used to retain them in position. Next day the external dressing was removed, but the loose pieces of gauze constituting the deep dressing were left in position, as the discharge had not penetrated beyond them. The patient's temperature remaining normal, and the pain having disappeared, the case was not again dressed until January 11th, when both superficial and deep dressings were removed. The deep dressing was found to be soaked with bloody and puriform discharge, but was quite inodorous; the inner layers of the external dressing were stained with discharge. The ends of the catgut drain which had been left protruding were found lying loose on the face of the deep dressing, and the points through which the drain had been inserted were soundly healed and the walls of the abscess cavity firmly consolidated.

On December 11th, 1876, my clinical clerk, Mr. Henry, amputated the second toe of a young woman's right foot for deformity. The operation, at which I assisted, was performed under spray; one vessel was secured by catgut; and a catgut drain, consisting of six threads, was inserted, the ends being left hanging out both above and below. The edges were approximated, catgut sutures were inserted, and antiseptic dressings applied. On the second day the superficial dressing

was changed, and on the eighth day the wound was uncovered for the first time since its infliction, when it was found to be entirely healed, except at a minute point where the end of the drain had been brought out upon the dorsal aspect of the foot. The ends of the drain were found lying loose upon the dressing; the catgut stitches had likewise become detached; there had only been a trifling amount of discharge; and there were no signs of inflammation, union having evidently taken place by the first intention.

Alexander W.; aged seventeen, was admitted to Jacob's ward for the purpose of having a supernumerary toe removed. On June 18th, 1878, my clinical clerk, Mr. Rattray, performed the operation under my superintendence. The wound, which was situated on the outer side of the left foot, was about two inches in length. Three vessels were secured with catgut; the ends of the ligatures, instead of being cut off short, were retained for the purpose of drainage, and they were brought out at the posterior or lower aspect of the wound. The cut edges were accurately approximated, and kept in position by means of sutures of oily catgut and horsehair; the usual dressings were then applied. During the operation thymol was used instead of carbolic acid. The outer dressing was removed on June 20th, and a fresh one applied, but the deep dressing was left *in situ* until the 29th. On that day the wound was uncovered for the first time since the operation, when it was found to be *completely* healed; the ends of the drain and the catgut sutures were lying on the face of the protective, the horsehair sutures were still in position, but no trace of discharge could be discovered in their track when they were removed.

This case was even more successful than the preceding one, for here union was complete and entire at *every* point—a result which surprised me greatly, for, on account of the removal of a little more skin than was called for, there was tension, not certainly in a great degree, but sufficient to make me doubt the advisability of attempting to obtain union by first intention. The result, however, showed that my fears were unfounded; and it proves, as far as a single case can, that, in cases where antiseptic treatment is employed, it is necessary to modify one's views concerning hitherto well-established axioms regarding the treatment of surgical wounds where tension is present.

The advantages of an absorbable over an inabsorbable drain, of a non-irritating over one whose presence gives rise to a certain degree of irritation, are many, and may, I think, be best appreciated by a consideration of the points wherein ordinary drainage-tubes are found objectionable. For this purpose I shall make use of the following quotations from Mr. Chiene's original paper, of which he has kindly permitted me to avail

myself. "Acknowledging the undoubted advantages of the drainage-tube as regards efficiency, I have long felt its disadvantages: for instance, its interference with rapid healing throughout the whole extent of the wound; the irritation it not unfrequently caused by its presence as a foreign body; the blackening of the protective, showing that irritating compounds were always present in the rubber, however pure; the tendency to regurgitation of air along the elastic tube during dressing, thereby increasing the danger of mischief passing into the depths of the wound; the necessity of dressing a case solely in order to shorten the tube; and the impossibility of being able properly to estimate the rate at which this should be done. These are self-evident evils, and their removal has for some time occupied my attention." Mr. Chiene next proceeds to relate the circumstances by which he was led to think of drainage by catgut, and adduces some most instructive and interesting cases as examples of its efficiency. After describing how it should be employed, and giving an explanation of its mode of action, he says:—"I beg to recommend as worthy of the notice of the profession the principle of drainage by utilising capillary forces through skeins of an absorbable material like catgut, if by its use the evils of the drainage-tube, already referred to, are got rid of. We may now anticipate a time when, with catgut stitches instead of silk, horsehair, or silver wire, catgut drains instead of india-rubber tubing, and chromic-acid gut fixing together the buttons instead of silver wire, it will not be necessary to uncover our wounds from first to last during healing, when the deep dressing need never be shifted, and when the outer dressing will only require to be removed when soaked with discharge. The amount of discharge, in its turn, will be reduced to a minimum by the use of an absorbable animal material like catgut, instead of a non-absorbable foreign body like silk, silver wire, or india-rubber."

The extracts above given show at once the theoretical advantages of this new method of wound-drainage. I have not yet had sufficient experience of it in major operations to enable me to assert that it is of universal application, but I feel satisfied that in minor cases it is *simply invaluable*, and if the experience of others should coincide with what I have myself seen, which I firmly believe will be the case, Mr. Chiene will be justly entitled to the gratitude of both surgeons and patients, and to the credit of introducing one of the greatest improvements connected with the antiseptic management of surgical cases.

The last case to which I shall direct attention at present is brought forward not on account of its interest as a successful specimen of extreme conservative surgery obtained by the use of antiseptic measures, but in connexion with the employment of

specially prepared catgut, which will be found useful for sutures in cases where a non-irritating and lasting material is desired.

H. B., aged sixteen, presented himself at the Aberdeen Hospital on June 9th, 1877, suffering from a compound fracture of his left forefinger. The wound ran obliquely across the finger, and was about one inch and a half in length. The fracture extended through the joint, between the second and third phalanges, the point of the finger retaining its connexion to the rest of the member by a small band of skin. A portion of the articular surface of the third phalanx had become detached from the bone. This was removed under spray, the wound was thoroughly washed with carbolic water, a drain consisting of two threads of common antiseptic catgut was inserted, the parts were brought into accurate apposition and stitched with chromic-acid catgut, a small splint was adapted, and gauze dressings applied. The patient was sent home, with directions to return next day. The wound was dressed from time to time; ten days afterwards it was firmly and solidly healed, excepting at the points of emergence of the drain, where there were two pin's-head granulating points. The patient had never suffered from pain; there was no inflammatory swelling from first to last, and the discharge was hardly worthy of notice. Two of the stitches were allowed to remain until June 30th, the antiseptic dressings being continued, and on their removal they were found to be in exactly the same condition as when inserted, the portions of tissue through which they had been passed, and in which they had lain for three weeks, were *perfectly dry*, no drop of discharge surrounding them, nor following their extraction.

Since I first heard Professor Lister speak of catgut specially prepared for sutures, I have given the subject considerable attention. The first case in which I employed a small portion of catgut so prepared was one of nævus in a child, where I had to remove a portion of the lower lip. The suture, which was passed through mucous membrane, was removed on the fourth day, and on minute examination, I found that it was unchanged in appearance. This I knew, from a considerable experience of ordinary antiseptic catgut sutures, was not what might have been expected had the material not been specially prepared, for the catgut which is in common use, though admirably suited for sutures, in very many cases becomes softened when exposed to the influence of much moisture, and, from this softening process, becomes detached at a very early period. As an example of this, I may refer to a case of excision of what was supposed to be a loose body in a knee-joint, but what proved to be, when the joint was laid open, a growth attached to, and forming part of, the synovial membrane, which was clipped off with a pair of scissors. The edges of the wound

were united by stitches of ordinary catgut, and, though there was no synovitis, and no suppuration, the gut became absorbed before union had taken place, and the part healed by granulation—a result certainly sufficiently good to satisfy anyone not acquainted with the behaviour of joint wounds under antiseptic management, but which might have been still better had the sutures not been so readily absorbable.

During the last few months I have conducted a number of experiments with catgut steeped in solutions of chromic acid of varying strength, the material being kept in the solutions for a given number of hours with a view to obtain a substance which, while capable of being absorbed, might be relied on as possessing sufficient endurance, if I may so put it, to last until its use could be dispensed with; but this I have as yet failed to obtain, the weakest solution of chromic acid seeming to so tan the catgut that its solution and absorption is rendered impossible. Still chromic-acid catgut sutures, similar to those used in the foregoing case, are of great value in a very large class of cases, and may be regarded as perfectly reliable substitutes for silk or wire, as, with the single exception of absorbability, they will be found to meet all the requirements of this mode of keeping cut parts in apposition, For chromic-acid catgut possesses the advantage of a maximum degree of strength combined with a minimum of size, and it is absolutely non-irritating. I have now used it in a great variety of cases, from major amputations to minute excisions, and in no case have I had reason to regret its employment. I do not mean it to be understood that in every case it has held the parts together until union has resulted, but that it has proved lasting and non-irritating in all; for example, in a case of amputation of the thigh, where union did not take place by first intention, the sutures soon became detached from the edges of the posterior flap, but some of them were allowed to remain attached to the edges of the tissues forming the anterior flap, without occasioning the slightest degree of inflammatory irritation. The mode of preparation I have hitherto found most reliable is this: I take a few yards of thin, common, unprepared, dry catgut, and place it in a wide-mouthed phial containing glycerine and carbolic acid (one part of acid to seven of glycerine), and, after steeping for a week or longer, the catgut is transferred to a vessel containing chromic acid, acetic acid, and water (one per cent. of chromic, and twenty-five per cent. of acetic acid.) At the end of seven hours the catgut is taken out and dried. During the drying process it is prevented from curling up, or getting rough and uneven, by using tension, the gut being stretched by winding it round two nails driven into a piece of wood, a foot or two distant from each other. In

the course of a few hours it will be found ready for use. Catgut, thus prepared, is a brownish black, perfectly smooth material, possessing great strength, yet not much thicker than what is sold in bottles as "fine" antiseptic catgut ligature. The mode of preparing tanned catgut may possibly be already well known; but, as I have not yet seen any description of it, it occurred to me that a detailed notice of it might prove useful to those seeking for a reliable substitute for the materials commonly employed for sutures.

In the conrse of this paper, which I must now conclude, I may perhaps have dwelt at undue length on the lesser points connected with this wide subject, to the exclusion of the greater and more important; still I am inclined to hope that a careful consideration of the cases adduced and the phenomena observed will not be altogether lost upon those who have not yet found themselves able to believe that the antiseptic mode of Professor Lister must displace every other method yet brought forward, and that there is something more in it than careful drainage and absolute cleanliness.—*Lancet*, *July* 20, 1878, *p.* 78.

ORGANS OF CIRCULATION.

36.—TREATMENT OF WOUNDS OF THE SUPERFICIAL PALMAR ARCH BY ACUPRESSURE.

By EDWARD BELLAMY, Esq., Surgeon to the Charing-Cross Hospital.

A perusal of the various English works on Surgery does not impress me that this simple method is practised as frequently as it might be. I record a case which occurred lately in my own practice. A lad, whilst cutting some toffee from a plate, cut his ulnar artery through, just at the point where it takes its bend towards the radial side of the palm (in the "line of fate"), and when brought to my house, was losing blood rapidly, per saltum, from a deep wound about an inch and a half long. I applied Esmarch's bandage, and endeavoured to find the bleeding points, but to no purpose. I then plugged the wound and bound the hand to a dorsal splint firmly, so as to get pressure on the vessel by means of the tension of the palmar fascia, and applied compresses over the trunks of the radial and ulnar vessels. He soon returned to me bleeding as profusely as before. I then determined on acupressure, and taking a stout harelip-pin, passed it through the tissues about half an inch from the edge of the cut, under the artery, and out again to a corresponding distance the other side of the wound, and placed the limb again on the splint. This had the effect of entirely

stopping the bleeding; the needle was removed on the fourth day, and the entire wound had closed by the end of the week.

I am well aware of cases of a like nature being treated by passing a hare-lip pin under the radial and ulnar at the wrist, but although it has been effectual, it is not without its dangers, and I contend that in cases of wound of the superficial palmar arch (and this is not the first I have treated similarly), acupressure at the point of injury should be resorted to at once.

As a matter of anatomical fact, the bloodvessels, as it were, cleave to the palmar fascia, even after a long escape of blood; and with ordinary anatomical precision, and a knowledge of the possible contingencies of irregularities, should be secured. A double needle might be used in some cases, to make quite sure—one on either side of the division in the vessel.

I need hardly say that these few remarks apply more particularly to wounds involving the superficial arch, although the "deep" arch is topographically not so deep or so ungetatable by this method as might be imagined.—*Lancet, Sept.* 21, 1878, *p.* 401.

37.—EMBOLISM OF THE BRACHIAL ARTERY FOLLOWED BY GANGRENE OF THE HAND AND FOREARM.

By Richard T. Gore, Esq., Bath.

Mrs. H., living about twenty miles from Bath, had a miscarriage at the latter end of November, 1867. When partially recovered, though a uterine discharge still continued, she was exposed to extreme cold at night. Up to Dec. 10th, she had suffered for some days very severe pain in the left hand and forearm. On Dec. 16th, it was found that all pulse in the radial and ulnar arteries had ceased, and the former could be felt at the wrist as a solid cord. Paroxysms of severe pain continued for several days, especially about the elbow. Sensation remained perfect, and there was very little reduction of temperature until Dec. 26th. At that time all pulsation had ceased up to the middle of the brachial artery, which formed a solid cord, with some thickening at the bend of the elbow. The nails and ends of the fingers now began to have a faint purple tinge; and there were two discoloured patches on the back of the hand. The return of blood by the veins at the bend of the elbow is very tardy. The pulse at the other wrist is about 90, and very feeble. The action of the heart is quite natural; the tongue red, and rather dry. A slight muco-purulent discharge from the uterus continues, but there is not any uterine tenderness. She sleeps fairly in the intervals of pain, which is not continuous.

Dec. 31st. She is considerably relieved from pain; the hand

and fingers are cold, reddish-purple, and shrivelled; the coldness and discolouration extend nearly to the elbow; no pulsation is to be felt in the brachial artery below the margin of the axillary artery, and there very imperfectly. Pulse at the right wrist 120. Tongue red, glazed, and aphthous.

Jan. 9th, 1868. In this interval she had much improved in health, taking food and stimulants in fair quantity. The pain has nearly ceased. The hand and forearm are dry, with a few blisters about the bend of the elbow, and a faintly-marked line of demarcation just above it.

11th. I amputated the arm about midway between the elbow and shoulder, making two flaps. The brachial artery and vein were blocked up with solid coagulum. Not any artery needed a ligature, and there was scarcely any oozing from the cut surfaces. The edges of the wound were brought together with wire sutures, and dressed with a strip of lint moistened in tepid water. Immediate union took place by adhesion, and henceforth recovery was rapid and uninterrupted.

The arteries in the hand and forearm were blocked up at all points, and the muscles shrunken, and of a dull purple colour.

No doubt in this interesting case the starting-point of the mischief lay in the congested state of the uterine vessels, accompanied probably by an unduly plastic state of the blood, leading to the formation of a clot and embolism of the brachial artery, forming in the whole a good specimen of dry gangrene, independent of injury.

The patient was under the immediate care of Dr. Bishop, of Calne, to whose diligent care and attention throughout her well-doing was greatly to be attributed.—*Lancet*, *Aug.* 24, 1878, *p.* 252.

ALIMENTARY CANAL

38.—ON THE RADICAL CURE OF HERNIA; WITH A CASE.

By Dr. GEORGE BUCHANAN, M.A., Professor of Clinical Surgery in the University of Glasgow.

The operation for the radical cure of reducible inguinal hernia is not one which is likely to be often performed by the ordinary practitioner of medicine; it belongs rather to the domain of the surgeon. The condition for which it is performed, is one which causes discomfort and trouble, but is not the source of immediate danger. The subjects of it are not likely to know anything about the operation, or even to have heard of it, but they are constantly referring to their medical attendants in consequence of inconvenience arising out of ill-fitting trusses and other annoyances inseparably connected with

the affection. When any case comes under the observation of the surgeon either in hospital or private practice, it is almost certain to have been previously brought under the notice of the general practitioner—so that in the first instance it rests with him to recommend his patient to undergo the operation. If by describing to you the nature of the operation, and proving to you by an example, and by the results of my own experience, that it can be undertaken with safety and success, I hope that the operation will soon be more generally had recourse to than hitherto.

Although reducible hernia is not a source of immediate danger, the unfortunate subject carries about with him a condition which at any moment may become so, and the case before us may serve as a very striking illustration.

James M., aged 57, ship-rigger, was admitted to the Western Infirmary on the 24th of September, 1877, with a simple fracture of the thigh in the middle of its lower third. The limb was put up in the long splint, and the progress toward recovery was uninterrupted, so that at the end of five weeks union had taken place. At this time a starch bandage was applied, in order to allow the patient to walk about the ward for a few days before dismissal. On the 12th November, after walking about, he was getting into bed, when the exertion caused the descent of a hernia of which he had not mentioned the existence before. Being foiled in several attempts to return it, he mentioned it to the nurse who at once sent for the house-surgeon. The hernia was on the right side, distending the scrotum to the size of a closed fist. Taxis was tried first without, then with chloroform, but, after twenty minutes pretty powerful pressure, it failed in effecting reduction. The house-surgeon then sent for me. I arrived about three hours after the hernia had come down. In the interval between the trial of taxis and my arrival, the patient got sick and vomited, and lay on his back a good deal exhausted and depressed. While so lying he laid the weight of his hand on the tumour and with a little manipulation it slipped up.

The man now tells us (19th Nov.), that sixteen years ago while raising a heavy bag to his back, he felt something give way in his right groin, and found that a protrusion had taken place—which has recurred every now and then, and is inefficiently retained by a belt: in fact he has a reducible inguinal hernia. On examining the canal I find I can push my finger easily through the external inguinal ring up to the very top of the canal, and the least attempt to cough causes a protrusion from the abdomen.

The recent protrusion, which was attended with considerable pain, and was very nearly accompanied by strangulation, has

widened considerably both the canal and rings, and so has made the hernia more easily protruded than before. The patient has got a fright, and feels that he is daily running the risk of his complaint becoming dangerous and even fatal—so that he has no hesitation in submitting to the operation for its radical cure.

The operation is as follows: The patient lying on his back with the knee slightly bent and raised, is put completely under the influence of chloroform. An incision an inch and a half long is made obliquely across the scrotum about three inches below the external ring. This incision divides the skin and dartos alone. After a few touches of the knife,—the finger-nail, and then the handle of the knife, can be used to separate the skin and dartos from the hernial sac and its denser coverings (which for convenience I shall call the *sac*). This separation is to be made all around the incision to the extent of a five shilling piece or more, according to the size of the tumour. The point of the forefinger is now to be put through the incision, against the lowest part of the sac, and, by a gentle shaking and pushing motion, is made to invaginate the sac (without the skin), up through the external ring into the inguinal canal, as far up as the internal ring. The finger is now lying in the canal in front of the cord. The tip of the forefinger is now shifted underneath the inner pillar of the external oblique aponeurosis and by a little manipulation can hook forward below that aponeurosis, the lower edge of the internal oblique just where it ends in the conjoined tendon. This can readily be felt as a prominent ridge against the external aponeurosis. While the tip of the finger is retaining the parts in the position above described, the surgeon takes in his other hand a very strong curved needle with an eye-hole near the point, and fixed in a strong handle. An assistant has near at hand about two feet of thick, strong, pliant silver wire bent double, so that the two ends are together, about half an inch of each end being bent into a hook.

The curved needle, well oiled, is now entered at the scrotal incision, slipped along the inner edge of the forefinger as far as its tip—that is the edge nearest the umbilicus—and is made to pierce the conjoined tendon, also the internal pillar, so that its point projects below the skin not far from the situation of the internal ring. The skin is now slipped over the subjacent structures as far as it can be drawn towards the umbilicus and the needle point thrust through it. One of the hooked ends of the silver wire is now put through the hole near the needle point, and by a rapid motion the needle and wire are drawn down along the edge of the forefinger and out of the scrotal incision. The needle is again entered as before and passed along the outer edge of the forefinger, and by it guided underneath the outer pillar of the external ring as far up as the

former puncture. The needle is now made to pierce the aponeurosis near Poupart's ligament, and its point rests against the skin, raising it up. The skin is pulled downwards and outwards, till the former puncture is opposite the point of the needle, which is then pushed through it. The other hooked end of the wire is put through the needle's eye, and by it pulled down the canal and brought out at the scrotal incision.

The next step is to pinch up the sac from off the cord, at the situation of the external ring, with the forefinger and thumb. The unarmed needle is now put in at the scrotal incision and passed through the sac, across the cord, below the edge of the internal pillar through which it is thrust, and made to project at the inner angle of the scrotal incision when it is pulled upwards to be opposite the needle point. The hooked end of the wire which was first drawn down is now put through the needle's eye, and by it drawn out at the point where the needle was inserted. An assistant puts his finger into the loop of wire which remains protruding from the hole in the skin of the groin, which was made by the punctures, and holds it fast, while the operator takes the ends hanging out of the incision in the scrotum and pulling them firmly straightens the wires as they lie in the canal. This stretching, done with some force, draws the two sides of the canal together, so that the external ring is tightened till it grasps the cord. The inclusion of the conjoined tendon, or lower border of the internal oblique, in the grasp of the wire, secures that all the boundaries of the canal are drawn closer.

The two ends of the wire in the scrotal incision are twisted firmly together up into the canal, and the loop at the groin is twisted down into the puncture, in each case three turns in the same direction being given to the wires. By this the sides of the canal are still further approximated, and part of the tissue entangled in the twist of the wires.

The loop of the wire is now bent down to the skin, and the wires with the two free ends bent up and passed through the loop and secured; pressure of the wires on the skin being prevented by a pad of lint. A little oiled lint is placed on the scrotal incision, a pad of lint and compress, retained by a bandage, completes the dressing.

The patient is removed to bed and ordered to have an injection of 60 drops of laudanum in about an hour, to be repeated at 10 p.m., and continued nightly for six or eight days.

Remarks and sequel.—The operation was contrived in 1861, by Mr. John Wood, Professor of Clinical Surgery in King's College, London; and the steps of the proceeding here described are those which were exhibited to my students on the 19th November.

The patient was kept lying on his back for ten days: the bowels were kept from acting by laudanum enemata; and the food ordered was concentrated and nourishing, principally beef steak and toast, and a glass of port wine.

After ten days the wires were removed, and the inguinal canal was found to be occupied by a firm plug, which quite filled it up. In a few days the apertures where the wires were drawn out were closed up.

At present (22nd Dec.) the valvular condition of the inguinal canal is completely restored, the sides being approximated, any interval being obliterated by the fibrous matter which had been formed where the wires lay; the bowels are acting naturally, and he takes the ordinary diet of the hospital. When he makes any attempt to cough the impetus is seen to affect the lower abdominal wall on both sides equally. There is no bulging or other indication of the former seat of the hernia.

From a perusal of Professor Wood's statistics, which include the practice of himself and several other surgeons, from the character of the operation, and from my own observation, I think, as a remedial measure attended with safety and success, it ought to be more generally had recourse to; at all events, when the wearing of a truss causes much annoyance, and where the truss inefficiently performs the service for which it is worn. —*Glasgow Medical Journal, Jan.* 1878, *p.* 1.

39.—THE TREATMENT OF RECTAL FISTULÆ BY IMMEDIATE LIGATION.

By H. A. Reeves, Esq., Surgeon to the Hospital for Women, and to the East London Children's Hospital, &c.

To divide fistulæ which run some distance up the bowel is, as is well known, a dangerous proceeding, on account of the serious risk of hemorrhage. Various methods have been devised to prevent this, such as the use of the écraseur, the galvanic cautery, the ordinary ligature, and the elastic ligature.

Some time since I adopted a method at the Hospital for Women, which, so far as I can ascertain, is new. It consisted in passing a strong and well-waxed silk ligature along the track of the fistula into the bowel. An ordinary surgical probe, with an eye at its end, carried this thread into the rectum. My bivalve expanding speculum was previously introduced, and by its use there was no difficulty in seeing and seizing the ligature and bringing it out through the anus. The probe was then withdrawn, and the ends of the silk were wound round two strong pieces of wood, which were held between the fingers of each hand. An assistant passed a finger on either side of the track of the fistula, to steady the tissues, and to resist the traction

which was put on the silk thread. The two pieces of wood were then drawn towards me with a rapid sawing motion, and the fistula was quickly divided with the loss of scarcely any blood. Some oiled lint and a pad and bandage were applied in the usual way, and the wound healed well. No anæsthetic was administered, and although the patient did not relish the operation, still it was quite bearable, and what she felt most was a burning sensation, due doubtless to the friction of the silk.

Since then I have rapidly divided a fistula which ran rather high up, by passing a strong piece of waxed twine, and tying the tissue till it was divided. This is somewhat easily effected by drawing on the ends of the twine until the parts included in the knot have given way. The vessels are crushed before they are torn through, and no blood, or the merest drop, is lost.

Either of these methods (but the latter by preference) can be adopted in any or all rectal fistulæ, and it is not absolutely necessary to administer an anæsthetic. It will solace the patient to know that he or she will not be cut; and further, the wound heals kindly, and there is no bleeding.

When performing the first of these operations, I remarked that I thought of this plan in consequence of noticing how easy it was to cut through a hard cheese with a piece of string; and I determined to try the method at the first opportunity. As I have pointed out, the proceeding was simple, speedy, and quite satisfactory.

I must add that the use of the speculum mentioned greatly facilitates the operation, because it allows the end of the probe carrying the twine or whipcord to be easily seen, and also because it steadies the lower end of the rectum.—*Medical Times and Gazette, June* 15, 1878, *p.* 649.

40.—ON PARACENTESIS ABDOMINIS AND THORACIS BY CAPILLARY TUBES.

By Dr. JAMES F. GOODHART, Assistant-Physician to Guy's Hospital and the Evelina Hospital for Children.

[The following cases illustrate the new method of draining fluid from parts by small capillary tubes.]

Case 1.—Susannah C., aged seventy, was admitted into Guy's under my care on April 17th. She was suffering at the time from extreme ascites with corresponding distress in breathing from the interference with the respiratory organs by the upward pressure of the fluid in the abdomen. Some months before she had been in the hospital under Dr. Wilks, and had experienced great relief from paracentesis, so much so that she said she had been quite well since. The fluid in the abdomen had re-collected lately. She was considerably wasted, but otherwise in fair

health. The viscera were all normal, except the liver, which was, in all probability, cirrhosed. The peritoneum was tapped by a Southey's cannula, introduced on the right side of the abdomen below the level of the umbilicus. An india-rubber drainage-tube was attached, and fluid allowed to run for about twenty-one hours. By this time some eight pints of ascitic fluid had drained away, and the distension was quite relieved. The abdomen was now bandaged tightly, and the patient left the hospital a few days afterwards.

Case 2.—Henry A., aged six, admitted into the Evelina Hospital under my care, by the kindness of Dr. Frederick Taylor. He had had rheumatic fever or scarlatina before Christmas, and soon afterwards his legs were found to be swollen. He was admitted for extreme anasarca of the face, trunk, and extremities, and with the abdomen painfully distended with ascitic fluid. The heart was large, and there was free mitral regurgitation, the urine being normal, but scanty. The peritoneum was tapped at once by a capillary tube introduced to the right of the umbilicus, that being the dependent side. An india-rubber tube was attached, and between 1 p.m. and 10 p.m., when the boy pulled out the tube, 6 pints 14 oz. had drained away. He was much relieved. He was ordered two drachms of the resin-of-copaiba mixture, and to this some tincture of of digitalis was subsequently added. Notwithstanding this the abdomen filled again. He was now tapped again in the same way as before, the cannula being left in the peritoneal cavity for forty-eight hours. Ten pints of fluid had drained away, the latter part of it containing a little thick ropy sediment. He had complained of a little pain during the last few hours, which was to be attributed, I think, to the fact that the peritoneum had become emptied of its fluid, and the cannula consequently rubbed against the intestinal coils. He had no other adverse symptom from the operation.

Case 3.—Mary F., with general anasarca and ascites, the result of capillary bronchitis and distension of the right side of the heart, was admitted under Dr. Moxon's care, and it ultimately became necessary to tap the abdomen. A capillary tube was introduced, as in the preceding cases, and the peritoneum drained for about twenty-four hours. Nine pints of fluid were obtained in that time. She has been relieved, and has suffered no inconvenience from the operation.

Case 4.—Benjamin H., aged twenty-six, came as an out-patient, with ascites to such an extent that he was unable to walk upright from the pain the abdominal distension gave him. He had also double pleurisy, and from this and other reasons the case was considered to be one of tubercular disease of the serous membranes. He was admitted under Dr. Moxon, and

paracentesis was at first performed in the ordinary way with a large trocar and cannula. He was much relieved, and left the hospital for a short time, after a long stay, for a convalescent home. He was then readmitted under my care in a similar state to that when he came first, the abdomen being again painfully distended. He was now tapped by a capillary cannula, and ten pints of fluid removed in the course of twenty-one hours, when the tube was withdrawn. He was much relieved, and has suffered no inconvenience from the mode of tapping adopted.

This method of performing paracentesis abdominis will be sufficiently clear from a perusal of the cases. Dr. Southey's proposal for the treatment of anasarca is borrowed and applied to the treatment of fluid in the abdominal cavity—that is to say, a cannula of very fine bore is passed into the peritoneum, and, by means of a long drainage-tube attached to it, the fluid is slowly drained away. In the first instance the tubes made for the purpose of draining the subcutaneous cellular tissue were used, and they answered very well in that particular case; but, doubting whether they would in all cases be long enough, Mr. Hawksley made me some of additional length—one inch and a half,—but in all other respects after Dr. Southey's pattern. These have been in use since. The tubes have been retained in the abdomen for varying periods, but generally for about twenty-four hours, and it will not often be necessary to continue the drainage for a longer time than that. Should it be otherwise it would probably be better to withdraw the tube and insert another at some other spot.

Besides these cases of ascites, I have treated in a similar way a suppurating hydatid cyst in the lower part of the abdomen and an empyema. Both drained for a few hours very well and the empyema was probably thoroughlydrained; the hydatid cyst, however, ceased to run, and other measures became necessary.

It is not to be expected that purulent fluids can often be treated advantageously by drainage through very minute tubes, and particularly in the chest, where the pus is liable to be thick. Still cases do occur in which such a plan is applicable, and there can he no harm in testing this as a preliminary measure. In pleuritic effusions of serous character slow drainage of this kind has, I think, several points in its favour, as compared with aspiration; and in peritoneal effusions, in which we can calculate with considerable confidence upon the limpid nature of the fluid, the advantages of paracentesis performed in this way are sufficiently obvious to make it a wonder that it is not more usually adopted. I will enumerate these shortly.

In the first place, there is the difference in the severity of the operations. In the one case there is the incision through the

skin, and then the passage of a large trocar. Both these stages, though not particularly painful, are, nevertheless, somewhat so, and, at any rate, unnecessarily so if a smaller operation will do as well. Paracentesis by a minute trocar and cannula is hardly more than the prick of a pin, and even a child does not mind it.

Secondly, the major operation is by no means free from the risk of peritonitis. I am not prepared at the moment to say in what percentage of cases of paracentesis after the usual fashion peritonitis is set up, but I know that the two conditions are frequently associated. Dr. Habershon, in 501 cases of peritonitis, gives fourteen as occurring after paracentesis. It is nothing to the point to say, as may be said, that other predisposing causes co-exist and should share the blame; or, rather, it is very much to the point as an argument in favour of reducing any additional risk to its lowest possible limit. It may yet be a question whether a small cannula lessens this risk, because what is gained by diminishing the size of the tube is lost, may be, by its prolonged retention. Still, there is *primâ facie* evidence, derivable from experience in other parts of the body, which goes to show that a tube will remain as a syphon drain in an otherwise closed cavity for a long time without doing any mischief, and upon that point I entertain no doubt whatever.

Thirdly, and this is the most important consideration of all, in the major operation the peritoneum is drained at once, and almost completely; in the minor, more or less of the fluid is removed gradually in the course of some hours. But except in occasional cases, where the respiration is urgently embarrassed, and immediate relief is necessary, it is not only not requisite, but probably it is harmful, to empty the peritoneal cavity suddenly. Shock is one of the risks usually said to attach to the operation of paracentesis abdominis, and that term would include a certain, though small, proportion of cases in which the sudden abstraction of many pints of fluid from the abdominal cavity interferes with the normal action of the heart, and syncope ensues; and another group in which the same abstraction of fluid so alters the condition of the circulation in the abdominal viscera that for the time being, and until this rights itself, there is more or less temporary stasis. All this is obviated by withdrawing the fluid slowly. But more than this, one of the main advantages of paracentesis is lost to us by this, our usual method. Under such circumstances the operation is not, and cannot be, employed remedially. We perform paracentesis when we can no longer help it, to stave off some worse trouble. But surely there are many cases in which a timely withdrawal of a certain amount of fluid and careful attention to strapping and bandaging of the abdominal walls at the same time would be followed by permanent relief and sometimes by

cure. *Per contra*, to empty the abdominal cavity at once is to favour the immediate transudation of so much more fluid, and to do away with the prospect of any than the most temporary benefit.

One caution is necessary in using small drainage tubes, and this is that the difficulty of cleaning them is in inverse proportion to the diminution of their calibre; and they should, I think, always be placed in some boiling germicide, such as a solution of carbolic acid, before their introduction.—*Lancet*, *Aug.* 24, 1878, *p.* 246.

41.—ON PARACENTESIS ABDOMINIS BY GRADUAL DRAINAGE WITH A SINGLE FINE CANNULA.

By Dr. REGINALD SOUTHEY, Physician to St. Bartholomew's Hospital.

The unusual relief of the distressing symptom of anasarca which I found follow the employment of fine drainage-cannulas encouraged me to employ nearly the same apparatus in the treatment of ascites; and now that my experience has extended over a fair number of cases, enough to satisfy me that this mode of proceeding is attended by no extra risks, I venture to lay it as briefly as I can before the profession.

Apparatus.—The trocar and bulb-headed cannula required for the purpose of gradually drawing off ascitic fluid, by the help of a capillary tube, differs very little from that employed by me for anasarcous limbs. Both instruments are equally fine. The calibre is the No. 1 exploring trocar of the surgeon. One long needle-trocar, measuring an inch and three-quarters in length from hilt to point, has appeared long enough for all the cases hitherto tapped by me. Three or four cannulas of different lengths, adapted to the thickness from fat and œdema of different abdominal walls, are required. The cannulas may be perforated with as many as six or eight side holes—the more the better, so that their strength is not interfered with. The mouth end of each cannula should be armed with a small silver plate or shield, to obviate any risk of the cannula head sinking beneath the surface of the skin when this is highly œdematous; and, simple as it may seem to contrive an armature which may thus secure and help to maintain the cannula in position, I may say that I have not yet quite mastered the matter. As to the length of the shield or cross-beam, one inch appears ample—*i.e.*, half an inch each side of the cannula. The shield may be round or square with rounded edges, or, as I have had mine made, as elongated plates, one inch long by a quarter of an inch broad, and about a thirty-second of an inch thick. Whether the cannula was best fixed immovably to the shield or otherwise,

was the next point to decide. It was found that the immovably fixed shield held fast by two strips of plaster, by dint of the movements of the abdominal muscles in respiration either worked away from its plaster moorings or tended to work out the cannula end from the peritoneal cavity. Messrs. Ferguson therefore contrived a shield for me which held, but allowed the cannula a limited play in every direction, and in practice this has worked admirably. One instrument I had made for those particular cases in which, although the ascites has been considerable, and its relief urgent, the presence either of cancerous tumours in the abdominal cavity or an eularged liver has rendered a hard and pointed body, like the cannula end, abutting on the peritoneal aspect of the abdominal parietes, undesirable; for as the fluid drains away the abdomen collapses and the parietes sink, and large, soft-surfaced masses, moved up and down by the descent of the diaphragm, might be torn and fretted against the cannula, and made to bleed. To meet this emergency a cannula which merely traversed a shield-plate, and was not fixed at all, appeared best adapted. If anything pushed it from within, out it could come.

Mode of operation.—Trivial as this is, it appears to me from experience that there is a right and a wrong way of introducing the cannula. Instead of driving the trocar in quite perpendicularly, it is best to slope the point downwards somewhat towards the pubes, and to avoid making the cannula point upwards towards the sternum. The wound made is so slight that one can afford to make it almost anywhere, but from prejudice I should select the raphe or mesial line below the umbilicus, and about midway between this and the pubes. Before operating, I always insist upon my house-physician ascertaining that the bladder is empty.

I append one case of ascites from cirrhosis thus treated, but for the last year I have had every case of ascites—hepatic, carcinomatous, cardiac, renal—which has fallen under my care at St. Bartholomew's, and required tapping, tapped in this way; and the results have proved sufficiently satisfactory for me to recommend it highly. I have had no instance of peritonitis thus provoked. The paracentesis, instead of being a formidable operation is nothing more than the prick of a needle. The ascitic fluid is quite sufficiently evacuated; it is also removed gradually, the near average rate of its removal being from ten to twenty ounces per hour. The pressure upon the diaphragm, the intestines, the intra-abdominal vessels, and the walls of the abdomen, is slowly but steadily relieved. There is no syncope provoked and no necessity for swathing the patient with bandages, a circumstance in the old method of performing paracentesis by large trocars which in hot weather

added greatly to the patient's distress. Both by doctors and patients this mode of performing paracentesis will, I think, be hailed as an advance in clinical medicine.

William H., aged fifty-eight, shoemaker, was admitted into Luke ward on March 12th, 1878, for extensive ascites and anasarca of his legs. The abdomen was very tense; dyspnœa considerable; some cyanosis. Urine scanty, high coloured, of high specific gravity, containing no albumen. Heart's apex beat two inches outside left nipple line; systolic murmur loudest at apex and over ventricle. Pulse very irregular. Breathing shallow; some œdema of both bases, posteriorly with bronchial râles. Limit of liver and spleen not to be ascertained by reason of the ascites. Up to ten years ago he had had good health; then had first attack of rheumatic gout. Was admitted into the hospital in June, 1876, for dropsy of legs and abdomen, and was discharged well, and again for a recurrence of dropsy about Christmas, 1876.

The man's physiognomy, his habits of life, and the manner in which his present dropsy had commenced, the abdomen swelling before the legs, led me, notwithstanding the cardiac murmur, manifest dilatation of both ventricles, and irregular heart's action, to attribute his dropsy principally to cirrhosis of the liver and an obstructed portal circulation.

Paracentesis abdominis with my fine trocar and fixed shield was performed at 6 p.m. on March 13th. In twenty-one hours, 11,400 cc. of clear, straw-coloured serum had been evacuated by the capillary tube. The specific gravity of the ascitic fluid was 1,020; reaction alkaline. The amount of fibrin as well as of albumen in it very considerable; the former manifesting its presence by spontaneous formation of a slight coagulum in the fluid. The tube was removed during the night of the 14th-15th, the fluid having ceased to flow, and the abdomen being quite flaccid.

March 18th. Condition singularly improved: appetite good; functions normal; urine flow abundant, clear, amber-coloured, alkaline, sp. gr. 1,024, no albumen; breathing quite tranquil; sleeps well; heart's apex still beats two inches outside left nipple perpendicular between fourth and fifth ribs; action still irregular; first sound loud and ringing, but attended by no murmur; liver manifestly contracted, small, and cirrhotic.

Towards end of April the abdomen had again filled considerably, and the legs began to be œdematous once more, but his appetite and general health were otherwise fairly good.

May 5th. The distension of abdomen and interference with the descent of the diaphragm again threatened death by dyspnœa. Breathing shallow and hurried. The auscultatory signs rendered presence of some fluid in right pleura probable,

as also œdema of lower part of left lung. Paracentesis again performed as before. The tube remained in thirty-three hours, during which time twenty-one pints of clear fluid were drawn off; it ceased flowing on the morning of the 7th of May.

On May 9th he was so perfectly comfortable and feeling so well that there was no object in keeping him longer in bed. On May 11th he was discharged at his own request.

Remarks.—This patient's case was doubtless a highly favourable one for relief by tapping. The best prognostic feature in any case of hepatic dropsy is a stomach that still maintains digestive powers. In treatment, however, this mode of performing paracentesis leaves nothing to be desired; the *tuto, cito, et jucunde* are sufficiently fulfilled by it.—*Lancet, Aug.* 10, 1878, *p.* 176.

AFFECTIONS OF THE EYE.

42.—A CASE OF BLEPHAROSPASM WHICH RECOVERED UNDER TREATMENT.

By Dr. THOMAS BUZZARD, Physician to the National Hospital for the Paralysed and Epileptic.

[The patient complained that at uncertain intervals the orbicularis palpebrarum muscle of the left side was spasmodically contracted, the eye being closed firmly for a few seconds. No apparent reason could be suggested.]

It seemed that one evening about two months previously he had suddenly felt a twitching in the left eyelids, which was accompanied by what he describes as a "numbing feel" about the left temple and ear. The next morning as soon as he began to eat his breakfast he got repeated spasmodic closure of his left eye accompanied by a sensation of dragging in the neighbourhood, which was very uncomfortable. And so this went on. Whenever he began to talk or to eat, or if he got excited about anything, there came a rapid succession of forcible closures of his eye for a few seconds. The result was that he shrank from talking for fear of starting the spasm, and that his nervous system generally had begun to be a good deal upset by the worry of this continually recurring trouble.

Asked as to any possible source of irritation at a distance, the only thing he could suggest was that within the preceding three or four months he had taken to wearing a truss which had occasioned him, as it often does when commenced somewhat late in life, a good deal of discomfort, until he got properly fitted.

Nothing likely to beget irritation presenting itself in his eye or mouth, I next proceeded to examine the face for "pressure-

points," *i.e.* for spots, pressure upon which would stop the spasm. No response followed pressure over points corresponding to the infraorbital, supraorbital, inferior maxillary, or subcutaneous malæ nerves, but on approaching the ear I found that when my finger was firmly pressed upon the tragus, and also on the skin for about an inch in front of it, the spasm ceased to take place so long as the pressure was continued, immediately recurring, however, and apparently with more than usual violence, when the finger was lifted. This observation drew my particular attention to the patient's ear. There was no deafness noticeable in conversation, but I noted that whereas he could hear Galton's whistle 8° with his right ear, he could only distinguish 5° with his left.

It appeared, too, that along with the numbing feeling complained of by him, there was an indescribable sensation within the ear, which the expression "singing" did not fairly designate. Examination with the ear speculum disclosed the presence of wax in the meatus, which was removed in large quantities by syringing. A feeling of increased comfort followed the discharge of the wax, but there was no appreciable diminution of the spasm.

I now applied the constant current from a Stöhrer's battery, placing one sponge rheophore (salt-water wetted) just in front of the tragus and the other behind the outer canthus, employing six cells (the battery had been quite recently renewed), and continuing the application during ten minutes. No attention was paid to the *direction* of the current, as experience has shown me that this is not important. The strength was lowered gradually to zero ere the sponges were removed, in order to avoid shock. A burning, not unpleasant, feeling was described as accompanying the application, and a good deal of redness of the skin remained after the removal of the rheophores. During the process the spasm of the lids was so greatly reduced in amount that it was scarcely perceptible, but it returned with force when the current ceased to be applied. When I saw the patient four days afterwards his condition had materially improved. There was only an occasional twitching of the lids, and he had quite lost the feeling of the eye being dragged down. The application of the current was repeated in the same situation. In three days more there was still further improvement, the spasmodic twitchings having ceased, and there being only a little, though constant, fine tremulous movement of the skin of the upper eyelid—that kind of peculiar localised tremor, indeed, to which the name "live blood" is given. The numbness or deadness about the left ear of which he had complained, had spread, he said, over the back of the head, and even as far round as the right ear. One of the rheophores was

applied to the eyelid with the effect of stopping the tremor apparent there. The application of a blister behind the left ear was followed by still further improvement: and when I saw the patient for the last time, a few days afterwards, he was going into the country, having nothing left to complain of except a very slight feeling of live blood in the lid. The hearing had improved in both ears; with the left he heard Galton 7°, with the right 9°.

Remarks.—It is known that a great many different sources of irritation of the fifth nerve may give rise to partial or diffused facial spasm. The most common example is doubtless shown by the effect of a grain of sand upon the conjunctiva in producing a very marked tonic spasm of the lids. In such a case it happens that the action is attended by pain, but this is of course not necessary, reflex action taking place quite independently of consciousness. It is this circumstance, indeed, which usually makes the search for the source of irritation difficult, and but too often unsuccessful. To Von Graefe we are especially indebted for indicating the frequency of the pressure points and the variety of situations which they may occupy. They are not necessarily confined to the surface of the face. Von Graefe mentions a case of bilateral blepharospasm in which pressure upon the left glossopalatine arch cut short the spasm. At this point there was an ulcer, and when this healed the spasm ceased.—*Practitioner, June* 1878, *p.* 404.

ORGANS OF URINE AND GENERATION.

43.—ON THE USE OF THE MICROPHONE IN SOUNDING FOR STONE.

By Sir Henry Thompson, Surgeon-Extraordinary to H.M. the King of the Belgians; Consulting Surgeon to University College Hospital.

What I have to show you here is something which will supplement our present power; but, I say it with the greatest confidence, the result of our present power with regard to lithotrity is, that we are able to remove to the last fragment with unerring certainty, provided the hands in which the case is are fairly (and only fairly) practised. I have demonstrated over and over again, both here and in hospitals abroad, the power of our present instruments to remove the smallest possible fragment. I have, over and over again, obtained an audible note from a piece no larger than a split pea, removing it afterwards to show the truth of the affirmation. You may say: "What more can you want than that?" Well, I think, not much; but let me confess that that is not at present an uni-

versally received opinion. There are many persons who still bring allegations against lithotrity, saying: "It is all very well; you may happen to do this, but but it does not lie within the usual practice of surgery to be so certain about that." It is still thought that it is a very easy thing to leave a piece behind. At all events, we will grant this, and we will grant still more, that if we can get anything which will make our task more easy, more simple or certain, it is our duty to add it to our present powers; and I think you will find that we have that in the apparatus before us.

But before showing you what the power of the apparatus is, I wish it to be most distinctly understood that this is not an instrument which is invariably to be used—no, not in more than one case out of twenty. Please understand that the stone is to be found by the ordinary method; and this is, I certainly will not say a surgical or scientific toy, a resource which we have in reserve for exceptional and difficult cases, or for those who may find it perhaps more useful than their unassisted hands and ears. It may be compared to a very high power in the microscope. I need not tell you what great discoveries were made with the microscope, in every direction throughout nature, with very low object glasses—glasses not to be mentioned in comparison with the high powers which we now use. It is a question how much those extremely high powers have done. They have really done very little. We have at present our fourth, our fifth, and our eighth, within the reach of ordinarily good microscopists; but, when you come to the twelfth and the sixteenth, they can only be used by highly skilled persons, and only here and there can such extremely high powers be required.

Now, the analogy between the microscope and the microphone is very strong. There are many things which we have been in the habit of regarding as without sound, as not producing a note. Nobody, for example, has ever heard a fly walking upon a window-pane. Most persons would say it was noiseless; but it is not so. It simply produces a noise which our unaided ears cannot appreciate. We cut our finger, and we see a little blood. The ordinary eye sees nothing but a little red fluid; nine persons out of ten would be astonished at seeing, under the microscope, what a composite fluid it is—the corpuscles, the liquor, and so on. Just so there are many notes in nature of which we have not the slightest appreciation, but by Professor Hughes's admirable invention we can hear and measure them.

With these observations, and wishing you to understand clearly the place in which I should put this relatively to our surgical armamentarium, I will show you what the microphone

is. Here is a battery, with the smallest section employed. If we had used a large section, it would have interfered with the success of the experiment. The current goes up to a telephone, then comes back here, and then goes on to another telephone. It is to be united with the sound, then with this wire, which goes to the other pole of the battery. It does not matter whether you begin with the positive or the negative.

I have here an ordinary sound for sounding a bladder and a microphone upon it. You see a little piece of gas-carbon balanced with a spring, and another piece lying transversely to it. Now, by striking anything with the point of the sound, an acoustic wave goes through the particles of the steel sound and comes up to this piece of carbon. This movable piece receives the impulse, and this now comes into circuit. But it is no longer an acoustic wave; it is transmuted into an electric wave. It suffices to touch a pin; the wave goes up; molecular changes take place which are acoustic in that piece of carbon, and there it ceases; they are then transmuted into electrical waves and are made audible by means of the telephone. Here is the secret of the matter: that, however tiny an acoustic wave is, it may be magnified the instant it becomes an electric wave. I connect these two poles, and, if I rub the two ends of the pieces of wire together, you can distinctly hear the sound by means of the telephone. [Experiment.] The circuit is now complete. I have here an extemporised bladder (a basin lined with wash-leather), and I put in it a small calculus. We will suppose that there is some suspicion that a portion of a stone is in the bladder, or a small fragment left in it. You may push it perhaps, but you cannot feel anything; but, with this instrument, if you touch with your sound a fragment, however small, your telephone will at once speak. I have used the instrument in the bladder, but, unfortunately, I have not a patient here to-day. If I touch but the point of a pin or a finger-nail, the sound is distinctly heard. Some trouble is required in order to get the microphone right for our purpose. In this respect, it is like the aneroid. If you want to measure certain heights with an aneroid—if you want to measure from the sea-level up to five thousand feet—you have one instrument for that purpose, and then for a second five thousand feet you require another instrument. One aneroid will not suffice for the whole range in the case of Mont Blanc, for instance, which is fifteen thousand feet; there you would require a mercurial barometer. The aneroid has only a certain limit within which it is correct. So it is with the microphone. I could put a microphone on this circuit which would be utterly useless. The microphone I have shown you would not enable you to hear the march of a fly. Professor Hughes says that he could make the march of a fly across a

piece of net sound like the tramp of an elephant. I have heard a fly walk across a piece of net, with a very audible note. In a public exhibition of the instrument, with an audience, say of one thousand or one thousand two hundred persons, a large telephone is used—a round trumpet-shaped thing that conducts the notes to the audience. Now, all that is very well in an ordinary lecture, but it is of no use in operating for stone. What I want to show you here is the way in which the instrument may be used in the chamber or hospital-ward. But, as I have said, this instrument is not delicate enough to make the march of a fly audible. If it were, when you put it into the bladder, you would have such a noise from friction from the urethra and bladder that you would not hear the little bit of stone. This is an illustration of the difficulty that often attends the use of delicate scientific things when you come to actual practice. This seems very simple when it is all arranged before you, but it has required some hours' trouble to get it right.

I have an instrument here which is fine enough and delicate enough to hear the sound of a fly; but, if the point of the sound were merely struck against the bladder, it would give a metallic note, just as if it had struck against a stone, so very sensitive is it. It must be borne in mind that you do not get a different note from the different bodies struck. The electric wave merely multiplies or magnifies the wave in the steel instrument; but it does not matter whether you have struck a blanket or a flint, or the mucous membrane of a bladder or a stone; the sound is precisely the same. [Experiment.] Here, if you have too powerful a battery, the friction-sounds would be augmented, and you will not succeed in your effort. Even the echoes of the room may interfere with your operation. I will now put the sound in the extemporised bladder, and, as I touch the little fragment of stone, you will hear a sharp click. It is in dirty thick water, and I cannot tell myself when I strike it; but those of you who listen through the telephone will perceive it.

In a few words, then, the demonstration is this: that you can make absolutely, logically certain the existence of small fragments in the bladder, for the detection of which you have hitherto depended upon your unassisted ear and hand. I want particularly to say that the unassisted ear and hand will suffice for almost all cases. There may be one in twenty cases in which it may be necessary or desirable to use this instrument. It is something like the case of the endoscope in regard to the urethra. When it was first introduced, a good deal was said about its being applicable to all disorders of the urethra and the bladder; but it was found in practice that, however well it

looked upon paper, there was only a case here and there that might be benefited by its use. What has mainly actuated me in bringing this subject before you is this. It is obvious that this new invention, which increases the sound of a foreign body, is equally applicable to a bullet or a shot, or any foreign body at the bottom of a wound or a piece of diseased bone. I thought it was desirable that this power should be examined carefully by one who was in the habit of doing something in this way, and it occurred to me that it would be agreeable to yourselves, and certainly gratifying to my own feeling, that the utilisation of the powers of this new invention should be first shown, not at Paris, not at Vienna, but in London, and, best of all, in our own Alma Mater.

I am indebted to Professor Hughes's kindness and courtesy for the microphones which I have exhibited, and for his aid and counsel in the matter.—*Brit. Med. Jour.*, *June* 8, 1878, *p.* 809.

44.—ON STRICTURE OF THE URETHRA.

By J. Cooper Forster, Esq., Surgeon to Guy's Hospital.

Of late years the pathology and treatment of stricture of the urethra have received much attention, and the extension of refinements in devising and applying operative means for its cure has been in proportion to the intellectual skill expended upon the study of this subject.

Causes of Stricture.—It is still a very commonly received dictum that gonorrhœa precedes and is the cause of stricture in a very large proportion of cases. Now there can be no doubt of the pre-existence of gonorrhœa in a large number of patients, but this hardly justifies the assumption that it is the cause of stricture. In the first place it should be borne in mind that gonorrhœa is so exceedingly common that it might readily appear that the two are closely associated, and yet without reason. On the other side there are no facts to show what proportion of cases of gonorrhœa escape stricture, although it is quite possible that this class is so numerous as to reduce considerably the significance which is often, no doubt, attributed to the would be cause and its effect.

The facts, indeed, on this point, which are at my disposal, do actually point in the direction which seems to me to be so probable. It is not often that an opportunity is afforded of visually examining the urethra in the course of gonorrhœa. I was, however, somewhat exceptionally placed in this respect, many years ago, on board the old "Dreadnought." Our sailors are proverbially prone to venereal affections, and when working at this subject I examined the urethra in a number of cases

where death had occurred from other diseases. Gonorrhœa was frequently present in these patients, but I never found in the whole length of the urethra anything more than simple injection of the vessels; no swelling, no hardness, nothing, in fact, much removed from the normal condition. I may even go further and say that in the few acute cases of gonorrhœa, which have been examined on the post-mortem table, very little more than this has been found.

Then, again, if I look over my cases of stricture, I find a certain proportion of patients, a small one it is true, who have had no gonorrhœa; a very large number who have had it many years before, and often without any severity; and quite a small proportion who have had any prolonged urethral discharge. There are, on the other hand, not uncommon cases in which the stricture has followed immediately upon the cessation of the discharge or even while it has still existed—in this way being exactly similar in their course to traumatic stricture. There can be little doubt that in such as these there has been some ulceration or sore on the surface of the mucous membrane, and that the consequent excess of submucous thickening has led to this immediate narrowing.

But by far the larger number of cases are those in which gonorrhœa has existed any number of years before the onset of the stricture, and this fact alone has been urged by some as an argument against the one being the cause and the other the effect. I do not however, agree in this, for the lengthened interval is capable of an explanation which has certain analogies with what takes place elsewhere, and which does in some measure link together even certain very remote conditions. The urine as it passes along the canal acts as a dilating force, and keeps the stricture from closing. Now, we all know that even after a stricture has formed, the very occasional passage of a catheter is sufficient to prevent the canal from closing, and the urine most probably acts as a dilator in the same way. Instances of other channels similarly situated with regard to fluids will readily occur to the reader, and I may mention the slow contraction of the valves of the heart after rheumatism as a parallel case. Another explanation of the chronic course which a stricture runs has been offered upon anatomical grounds. It is admitted by all that the common seat of stricture is in the bulbous or membranous portion of the urethra. It has been said that gonorrhœa causes a chronic urethritis at this part, but why here more than at any other it is difficult to see. It has been lately suggested that this spot is horizontal in position, and the secreted fluid behind and above it drops down into this part, and would set up and prolong any irritation. But while allowing that anatomical considerations rather than patho-

logical must determine the reason of the locality of the common site, I do not consider that the anatomy of the part or the changes of position which the urethra undergoes in the shifting postures of the body allow of any such division of the urethra, or permits the formation of a horizontal reservoir, such as Mr. Gould so ingeniously describes. I do not, however, doubt, notwithstanding these objections, that any one who has gonorrhœa is very likely, probably more likely, to intensify predispositions which exist in a normal urethra, if there are such; but as the inflammation will be acting along the whole course of the urethra, I fail to see how it can tell alone upon the membranous portion. It will then still be necessary to look to some local conditions to explain the actual stricture.

What, then, is the more generally applicable exciting cause? What are these local conditions? They are not far to seek. The urethra is a flexible tube fixed and narrowed at one point, viz., as it passes through the triangular ligament. It is about this part that the stricture occurs in the great majority of cases. It is obvious that the fixing of a tube, such as the urethra, at any one part of limited extent has certain disadvantages, which must prejudice its action and render every male urethra, so it seems to me, more or less liable to stricture.

The stricture segment, so to speak, is, as it were, poised between the cavernous tissues in front and the prostatic part behind, and in all the changes of position which occur in erection of the organ, extreme or moderate, there is a certain amount of stretching and dragging about this spot.

Nor does such an explanation, as may at first sight seem, prove too much, nor commit every one to stricture. Anatomical considerations show a *predisposition* to stricture; its actual occurrence will be determined or not by the moderation or immoderation with which the organ is committed to its functions. It is clear that those who suffer from gonorrhœa are just those who have in all probability sinned largely on the side of excess. This view is one which has not, I think, received sufficient consideration, and I commend it to those whom it may concern. I would further say that my own impression is that too frequent sexual intercourse is a more potent cause of stricture as the middle or later periods of life are reached, inasmuch as this is the only explanation I have been able to attach to certain cases of stricture past middle age, in which, so far as I could ascertain, and as I quite believe, no gonorrhœa had previously existed.

Symptoms.—I only wish to mention here two practical points. One is that a large number of patients are said to have stricture, and perhaps, as a consequence, think they have, but who have nothing of the kind, or at least, in whom a No. 6 or 7

catheter can be easily passed. Some surgeons are in the habit of considering the male urethra as equal in size to a No. 12 catheter, and from that standpoint cases such as I have named are cases of stricture; but surely the largest instrument that can be passed along a dilatable canal is no gauge of its normal calibre, and common sense tells decidedly that a much more moderate-sized instrument, such as a No. 6 or 7, is about the average of the normal urinary stream. If there be a permanent passage of this size there is no need for anxiety on the score of stricture. From this it follows, according to my view, that any symptoms of stricture that may exist under such circumstances—irritable bladder, twisting of the stream, inefficient expulsion of the last few drops of urine—arise, not from direct local disease, but from transferred irritation, of which the source is, by no means least commonly, cerebral, or rather mental. I would here also allude, although it is well known, to the dribbling of urine which occurs in bad cases of stricture, and which is the running over of the full cask. This is often overlooked. It is, perhaps, hardly likely to be so if the act of micturition is watched by the practitioner himself, but it is often only described by the patient under such terms as a weak bladder, or that the urine passes very freely, and so on. Under such circumstances the urinary passages should always be examined.

I would only further add, to avoid the possibility of misconception, that although it is true that many patients think they have stricture when they have not, the converse also is quite as true, that even in some extreme cases of stricture, if only casual inquiries be made, no complaint of any difficulty in passing urine will be elicited.

Treatment.—My object in writing this paper was chiefly to enforce more rigidly than the tendencies of late years would appear to indicate, that the old practice of dilating a stricture by simple catheterism is the most useful, the most simple, and the most sure of all the plans that can be adopted. I may say, judging from the experience of many years past, both in hospital and private practice, that I have scarcely ever seen the necessity of having recourse to such measures as external or internal urethrotomy. The latter operation I have never performed. The several advocates of these various operations may perhaps reply that the experience of any one man is limited. Yet surely the practice of a large hospital should offer an opportunity, now and then at any rate, of performing some cutting operation. All I can say is that I have only once performed section through the stricture, or, as it is called, external urethrotomy, during the last seven years, and then I regretted it afterwards. The plan of treatment I adopt is as

follows:—A patient applies for retention of urine and no catheter can be passed. He is placed in a warm bath, and a good dose of opium is given. In many instances no other measures are necessary. And if this holds good for the relief of retention of urine, which, although dependent in part upon paralysis of the muscular coat of the bladder, is certainly in part also, in most cases, dependent upon a stricture, it might be expected that the same treatment would relieve the stricture itself, and this is really the case. Therefore, *in all cases*, if upon admission no catheter can be passed by the most moderate manipulation, the treatment above named is continued, coupled with a reduction of diet and a complete absence of stimulants. No further instrumental interference is attempted for some four or five days, and at the end of that time it is rare to find any stricture resisting the passage of a small-sized gum elastic catheter. I scarcely ever use any other form. Should I even then be unable to pass an instrument, still I wait; the same treatment is continued, and a smaller catheter is afterwards employed. Sooner or later this is successful, and when once the catheter is introduced into the bladder it is tied in and urine allowed to run away through it, or, as very often happens, by its side. Notwithstanding all that has been said and written and done in favour of other methods, this is the one which hardly ever fails, but its successful application requires not only skill but patience and practice. Any rough usage to the canal will destroy the chances of success, and it will be no waste of time on the part of the surgeon if half an hour or an hour even be occupied in the most gentle manipulation, if at the end of that time a catheter is passed. Once in the bladder the battle is over, and the successful issue of the case is in the surgeon's hands.

Of course there are drawbacks; there are to every plan. First, there is the loss of time, then the confinement to bed. There is a little risk of cystitis and the expulsion of the catheter, if introduced too far into the bladder, while in some people there is an irritable urethra, which abhors the prolonged presence of any instrument. The first two ought not to be allowed to weigh against a successful mode of treatment, to which attaches little or no risk, when it is compared with other methods, which, however successful, are not so free from risk to the patient. The last two can usually be managed by tact.

Of the time required by this treatment it is necessary to say that it varies in different cases considerably. In some, and these the worst, with the greatest patience and care no catheter can be passed for some days, but these are quite exceptional, and if the surgeon has his patients under his own control, in other words, if he himself passes the instrument, the time should not often extend over ten days to a fortnight. The stricture

generally dilates rapidly when once a catheter lies within it. With regard to actual facts my hospital cases may not show well, but this is attributable to several causes which are easily obviated in private. Chief of these, it is almost superfluous to state, is the necessity of educating the future generation of surgeons, which can only be done by actually exercising them in any requisite manipulations. It cannot be too often insisted upon, when speaking of hospital cases, and even when comparing one set of cases with another in hospital practice, that the surgeon is, or ought to so consider himself, in the light of a consultant to his dressers as well as a teacher. The patient by this means loses a little time, but the gain is immense to those who thereby acquire in the usage of instruments tact and dexterity, which are nowhere more necessary than in manipulations of the male urethra. My relations with my dressers have always been determined on these utilitarian principles.

Sir Henry Thompson remarks that "Every surgeon who possesses tact, patience, and judgment, and, of course, a requisite experience, may undoubtedly treat successfully by his own favourite method, whatever it be, a large proportion of the cases that apply to him." But he adds, "let not his success lead him to imagine or to persuade the world that his method is the only true one." With this I, of course, entirely agree if Sir Henry only alludes to the varieties of simple dilatation, but if he includes the varieties of operative measures by the use of the knife or caustics, then I must also say I disagree with him, inasmuch as I consider all such means unscientific from a pathological point of view. I do not allude here to any methods of treatment which aim only at getting rid of the urine behind the stricture. I am only concerned with those operative measures which strive after *cure* by division or destruction of the stricture. All such appear to me not only unnecessary but unsound. For, if it be allowed that all cases of simple stricture, with hardly an exception, can be treated by dilatation, and that this is so is evident by the passage in most cases of a fine stream of urine, what more can be obtained by cutting through the stricture? If a Syme's grooved staff can be passed along the passage, so can a catheter, and if a catheter then the stricture can be dilated. So surely as a knife is used, however, we have to do not only with the stricture but also with the new tissue engendered by the cutting. This must be so, and cicatricial tissue will always contract. Any such fresh addition of fibrous material to that already existing must in the long run be a very positive disadvantage, and what advantage counterbalances this it is hard to say. A large-sized instrument can be passed at once and that is all. But there are other attendant risks whenever a knife is used; there is extravasation of urine, to say

nothing of pyæmia, erysipelas, or traumatic fever, for, be it remembered, these cases cannot be conducted on antiseptic principles.

Moreover, in relation to time, a much longer period elapses in the healing of the incisions made by the surgeon than would be required for the dilatation of any stricture. But I am told that some strictures are too dense, too cartilaginous to dilate. I cannot say that I have met with any which have resisted the presence of a gum elastic catheter within them, and though some may contract again rapidly afterwards, the same thing happens when any cutting operation has been adopted. Many of the same objections hold good for forcible dilatations; in these also tearing and bruising of the part is caused, and it cannot be otherwise than that fresh inflammatory material is added to that already existing, and the ultimate amount of material forming the stricture increased rather than lessened. There is no evidence, so far as I know, from the recorded after experience of the various methods of treating stricture, and certainly none, viewing the matter pathologically, which shows that those cases treated by cutting contract less readily than those not cut, and I do not for one moment believe that the after results differ in any way for the better. If all this be correct can we adopt a more simple or a safer plan than that of dilatation, slow or rapid according to the condition of the stricture.

But undoubtedly many cases have been brought forward—all of us have seen many such—in which the urine passes only guttatim, or at most in only an occasional stream. In such as these it is now and then impossible to pass a catheter. When this is the case I see no objection to the operation known as Cock's, viz., to open the urethra behind the stricture. But the principle here is a quite distinct one from those I have previously discussed. The stricture itself is not touched. The operation is performed to divert the stream of urine, and so to procure rest for the stricture. As a result one of two things happens; either the stricture relaxes sufficiently to allow, as is most frequent, the passage of an instrument, or it closes altogether, and the fistula made by the operation becomes permanent. I need only further add that I have not included fistulæ in perineo and traumatic stricture within the limits of the plan of treatment I have urged. In many such cases operative measures in the way of cutting the stricture are advisable or necessary. I have, as will appear quite plainly, only alluded to the treatment of ordinary simple stricture. I have perhaps written rather strongly because, as I said at the outset, I am much impressed with the idea that the present tendency towards cutting and other heroic methods of treatment is a mischievous one, and undertaken without due regard to the success which

follows the old and more simple plan or to the risk run by the use of the knife in any shape. The importunity of patients should not be allowed to determine their risks. I have said nothing that is new, but it is as much the duty of a teacher to repeat old truths as to start new ones which, from however reliable a source they come, must at any rate want the test of time to try them. Here is my experience in the matter, and I cannot do better than close this paper by showing in how close agreement I am with my respected colleague and former teacher Mr. Cock, who thus writes: "Some ingenious mechanical contrivances cut, some tear, some burst, some are supposed to dilate with marvellous rapidity. My objection to their use is founded upon forty years' experience, which has taught me that such cutting, tearing, bursting, or rapid dilatation, is often exceedingly mischievous in its effects, and fails in establishing a permanent cure, and that the object in view may be accomplished by much milder, surer, and safer means."—*Guy's Hospital Reports*, 1878, *p.* 173.

DISEASES OF THE SKIN.

45.—CHRYSOPHANIC ACID IN PSORIASIS: NOTES OF SIX CASES IN WHICH IT WAS EMPLOYED.

By Dr. J. C. OGILVIE WILL, Surgeon to, and Lecturer on Clinical Surgery at, the Aberdeen Royal Infirmary.

The results obtained in the following cases of psoriasis from the use of chrysophanic acid ointment have produced so strong an impression upon my mind regarding the efficacy of this mode of treatment, that I gladly comply with a wish expressed to me by Mr. Balmanno Squire that I should lay the particulars of them before the profession. Five of the cases were treated in my wards in the Aberdeen Royal Infirmary, the sixth was a private one. The effects of the remedy were watched with great interest by a large body of students, and I had also the pleasure of showing some of the patients to several brother practitioners, who all expressed themselves as feeling gratified with, and not a little astonished at, the rapidity of the progress of the cases, and the excellence of the results attained.

Case 1.—J. W., aged fourteen, was admitted on April 25, 1877, suffering from psoriasis. The arms, thighs, and legs were covered with innumerable scaly patches, varying in size from a minute point to that of a shilling, and presenting the characteristic appearances of *P. punctata*, *guttata*, and *nummularis*, most marked on the extensor aspects, but also involving a considerable extent of the flexor aspects. His chest, abdomen, and back were very plentifully studded with nummular patches, and his scalp

was in a similar condition. He complained of intense itchiness. On April 28, chrysophanic acid ointment—fifteen grains to an ounce of hot lard—was prescribed, directions being given that it should be well rubbed into the affected parts night and morning. Three days afterwards the scales were peeling off freely, and the itching had ceased. On May 3 the scaly patches had completely disappeared from the greater portion of the surfaces involved, and the infiltrated cutis was fast resuming its normal consistence. On May 7 the whole body was perfectly free from scales, and was dyed of a dusky purple colour; while the spots where the disease had existed presented a smooth, white appearance, forming a marked contrast to the surrounding discoloured skin. The head was shaved on May 3, for the purpose of allowing the application of the ointment to the scalp; but the shaving caused so much irritation that the use of the acid was delayed until May 9, when it was applied, the effects on the eruption being similar to those seen after its application to the body. On May 10 the epidermis covering the unaffected portions of the body was found to be exfoliating, the whole surface being covered by fine furfuraceous scales; but after the use of warm baths, the skin speedily became perfectly normal in appearance, no trace of disease remaining. For the purpose of observation, the patient was kept in hospital for some time longer, and on one occasion it was deemed expedient to reapply the ointment to his back, which presented a somewhat suspicious appearance, but on June 4 he was dismissed with a perfectly healthy skin. As a precautionary measure, he was directed to take small doses of Fowler's solution for a few weeks.

Case 2.—J. B. (male), aged seventeen, became an inmate of the Aberdeen Infirmary in July, 1877. He was the subject of well-marked nummular psoriasis of eighteen months duration. The patches were comparatively small in size, the largest being a little larger than a shilling; but they were abundant, the legs, arms, forearms, abdomen, and back being freely studded with them. The pearly-white appearance of the heaped-up epidermic scales was exceedingly well marked, but the thickening of the corium was less than usual. Chrysophanic ointment was prescribed. After the fifth application the scales were found to be much less firmly attached than before, and on the sixth day after treatment was commenced most of the scales had become completely detached, and those that remained were loose, and easily rubbed off by the use of slight friction. Two days afterwards the whole of the scales had disappeared, and the infiltration was much lessened. He was directed to have a warm bath, and to continue the ointment. In less than three weeks after the commencement of treatment, every trace had disappeared, his body presenting the usual appearance observed after the use

of this remedy—viz., white circular and oval patches of perfectly supple skin, occupying the points previously infiltrated and covered with scales, while the parts uninvaded by disease had assumed a prune-juice colour from the effects of the acid upon the normal integument.

Case 3.—M. G. (female), aged seventeen, sent to my care by Dr. Brander, admitted on October 26, 1877, suffering from *psoriasis diffusa*. She stated that she first observed the eruption on her knees about six months previously, the disease spread rapidly, chiefly affecting the extremities.

Condition on admission.—On the left leg the largest patch, situated right over the knee, measures $3\frac{1}{2}$ inches in length by $3\frac{1}{4}$ in breadth; it is plentifully covered with large, thick, pearly-white, firmly adherent scales, and the skin is much reddened, and thickened. Immediately above the knee there are three patches about the size of a shilling, and all over the anterior aspect of the leg there are patches varying in size from small spots to that of a half-crown piece. Right leg: a large patch of irregular shape, $3 \times 3\frac{1}{4}$ inches in size, over the knee, and another, $3\frac{1}{4} \times 2\frac{1}{4}$ inches, below the patella; numerous patches of smaller size on front, sides, and calf. Left arm: over elbow long patch measuring $5 \times 1\frac{3}{4}$ inches, and many smaller spots on arm, fore-arm, and hand. Right arm: on posterior and outer aspect of arm large patch measuring $3\frac{1}{2}$ inches in length, and the same in breadth; another over elbow-joint extending down fore-arm $6 \times 2\frac{1}{2}$ inches in size, and another large one on the back of the hand. Many small patches scattered over the fore-arm. In all the larger patches redness and thickening of the corium excessive. Treatment was commenced on Oct. 27 with chrysophanic ointment.

Nov. 1. Scales beginning to fall off from larger patches—lesser ones quite free from them. Nov. 3. Infiltration of smaller patches much lessened; still some scales adherent to larger ones. Unaffected skin dyed deep red colour. Nov. 6. All the scales have disappeared; infiltration much diminished. Nov. 10. Smaller patches beginning to assume characteristic white appearance; skin soft and pliable. Infiltration of larger patches disappearing, but still very manifest. Directed to have warm bath; strength of ointment to be increased to twenty grains to the ounce. Nov. 19. Redness of patches quite dissipated; still some thickening. To have a bath. Dec. 1. No trace of eruption on any part of body. (For the notes of the above case I am indebted to Mr. G. Rae, M.A.)

Case 4.—A girl, aged twelve, who had been admitted suffering from ranula, was found to be affected by psoriasis, her legs, knees, and arms being plentifully covered with small scaly patches. Duration unknown. Chrysophanic ointment was

prescribed, and was applied night and morning for ten days, when the disease had completely disappeared, and the patient was dismissed from hospital.

Case 5.—C. D. (female), aged thirteen, recommended by Dr. Brander, suffering from *P. nummularis*, especially affecting the knees and elbows. but also involving to a considerable extent the rest of the body. The disease had only been observed for six weeks, but was exceedingly well-marked. The patches varied in size from that of a mere point to that of a five-shilling piece; infiltration considerable, especially in the neighbourhood of the knees. The usual ointment was prescribed. Progress was exceedingly rapid, for at the end of a week all the scales had exfoliated, and the thickening had much abated; and at the end of three weeks she was discharged from hospital quite well, the whole skin being perfectly normal in appearance and consistence.

Case 6.—A. G., grocer's assistant, aged twenty-two, consulted me on November 13, 1877, concerning psoriasis, from which he had suffered for many years. When stripped the parts found to be most affected were the front of the legs, upper part of thorax, and fore-arms, where many patches varying in size and shape, but mostly about the size of a half-crown piece, and circular in outline, were observed. The face and neck and the dorsal aspects of the hands and wrists were studded with smaller spots; and these spots, from the prominent situations they occupied, gave the patient much annoyance. Nummular patches were also found on the abdomen, back, and other parts. The scalp was also affected. There was much thickening of the skin, but very little redness.

Chrysophanic ointment—twenty grains to the ounce—was prescribed. This was freely rubbed into the whole body night and morning. The scales speedily became partially detached, and soon commenced to exfoliate; but after the ointment had been used for ten days, considerable inflammatory redness of the skin, accompanied by heat and tingling, set in. These symptoms were specially noticeable in the axillæ and groins, where the skin was very much inflamed, and markedly hotter to feel than normal. The skin covering the front of the chest and abdomen was also hot, red, and tender. The other parts of the body were deeply tinged with the acid—the face being dark brown—but they were not irritated. He was directed to take a warm bath, to discontinue the use of the application to the parts most affected by redness, to apply the ointment carefully to each diseased patch, and to avoid inunction of the surrounding skin. The heat and tingling were much relieved by the bath, and in the course of a few days the inflammation subsided. This was followed by complete exfoliation of the epidermis covering

those parts of the body where the dermatitis had existed, the epidermis peeling off in large thin flakes; and the same process took place on his face, but it was slower, and the exfoliation was furfuraceous in character. At the end of three weeks from the commencement of treatment nearly every trace of disease had disappeared; the small spots on the wrists were the most obstinate, for at this period (Dec. 3), although the infiltration of the larger patches was completely dissipated, a very slight degree of undue thickening was still discernible at the points referred to.

On December 3, when the patient last presented himself, no trace of disease remained on those parts to which the remedy had been applied. He was enjoined to take arsenic as a prophylactic, and for the cure of the patches on his scalp to which the ointment had not been applied, for as he was on the eve of sailing for a foreign country he was unable to carry out the treatment of the *psoriasis capitis*, as I declined to allow him to apply the ointment to his head without the hair being first removed.

It will, I think, be allowed by all that the mode of treatment adopted in the foregoing cases was rapid in its effects, and satisfactory in its results; I therefore consider it unnecessary to occupy space with remarks on the progress of the cases, more especially as I have in the more aggravated ones given the details both as regards the extent of the disease and the effects produced by treatment at considerable length. One point, however, seems to me to be deserving of special notice—viz., the strength of the ointment employed, as it may be observed that instead of thirty grains to the ounce (the most usual strength prescribed), half that quantity of the acid was found sufficient. This is a matter of some moment, as the remedy is a somewhat expensive one, and when used in a concentrated form it is extremely irritating, especially to some skins, as was shown by A. G.'s case, where a somewhat stronger preparation was employed; as the weaker ointment seems to be all-sufficient, it should receive the preference, increasing the proportions of the acid should any case be so aggravated and obstinate as to call for that procedure.

Regarding the lastingness of the cure, I am unable to supply any data, but of its permanence I am somewhat sceptical, for experience has shown that, even in cases treated by internal remedies, such as arsenic, unless the medicine be continued for a long period after every vestige of eruption has disappeared, a relapse is certain. If even only temporary, still the gain is a great one, for we are now in a position to ensure any patient suffering from psoriasis a certain freedom from the eruption in the short space of a few weeks, and that too without the danger

of inducing arsenical, phosphoric, or carbolic toxæmia; while the recurrence of the disease may be prevented by the exhibition of small, non-toxic, but long-continued doses of arsenic.

There are, however, certain disadvantages attending the use of chrysophanic acid (two of which were brought forward as arguments against its employment at a recent meeting of the Clinical Society of London), and which may now be briefly noticed—viz., (1st) its irritant action upon the skin; (2nd) the staining of the skin; and (3rd) the dyeing of the bedclothes.

The first objection has been already mentioned, and I fail to see that it is more tenable in the case of chrysophanic acid than in that of any other medicinal irritant. In only one of the six cases naarated did any inflammatory trouble arise from the effects of the acid, and although I have employed it in a large number of cases of *Tinea circinata*, in no case has the acid occasioned any inflammation of the skin, and I feel satisfied that, unless in very exceptional cases, the occurrence of any undue degree of dermatitis can be readily averted by commencing with a weak ointment as already suggested.

The second drawback—the staining of the skin—is not so easily disposed of, for no method of extracting the dye from the epidermis has yet been devised; but I have not found even in private practice that this was regarded by the patients as a serious objection, for, as the curative effects of the agent were so manifest, the one point more than counterbalanced the other, The shedding of the epidermis, when assisted by warm baths, is not a protracted process, and at its conclusion the skin will be found to be purer in colour than before the commencement of treatment.

The third objection—the discolouration of any article of clothing with which the acid comes in contact—must fall to the ground, now that we know how to remove the stains; but when first employing Goa powder, from which chrysophanic acid is derived, and which is equally powerful as a dye, this objection seemed to me to be a somewhat serious one, for the matron of the infirmary complained to me that not only were the bedclothes belonging to the bed in which the patient slept deeply stained of a purple colour, but that nearly all the bedclothes in the ward were in a similar condition. On inquiry I discovered that this arose from the fact that the patient—a young lad suffering from favus—had a strange proclivity for standing on his head on the beds of the other patients. It is needless to say that his acrobatic performances were very soon put a stop to, and since that time we have always employed the same bedclothing for each fresh case, the articles being rendered practically, though perhaps not esthetically, pure before the reception of the new occupant. The necessity for such conservative measures

does not now exist, for Mr. Balmanno Squire has recently informed me that by the careful use of bleaching powder the stains can be readily got rid of, and thus the third objection has been satisfactorily overcome.

In bringing my remarks to a conclusion I would say that, from a not inconsiderable experience of the various modes hitherto employed for the treatment of psoriasis, I feel justified in asserting that by no other method can such certain and speedy results be attained as by chrysophanic acid; and as I accept without reserve Mr. Balmanno Squire's statement, that "the efficacy of chrysophanic acid in psoriasis is certainly one of the most astonishing facts in modern therapeutics," I cannot but think that this ready method of treating a notoriously intractable disease must ere long receive the approval of all unprejudiced observers.—*Practitioner*, *June* 1878, *p.* 415.

46.—ILLUSTRATIONS OF A NOVEL AND SUCCESSFUL TREATMENT OF PSORIASIS.

By Dr. JAMES ADAMS, Glasgow.

It is a common explanation and a common belief that those diseases of the skin which afflict with the most unconquerable tenacity owe their stubborn qualities to some mysteriously morbid condition of the blood—some constitutional bias—which can only be expurgated or rectified by internal remedies, *i.e.*, the *hæmato-cathartics*, which, acting upon the skin, bowels, and kidneys, eliminate the morbid leaven through these channels. And orthodox courses of arsenic, mercury, antimony, iron, &c., alternately with drastic purgatives, are almost necessary elements in the treatment, supplemented though they may be by filthy baths, restrictions in diet, changes of climate, &c. Of the usually inveterate diseases, among the most obstinate, most loathsome of aspect, and most destructive of the general comfort of the sufferer, is psoriasis, or the common English leprosy. And until the present time I have always accepted the conduct of these cases with great reluctance, and got quit of them, I fear, with little of satisfaction to myself or to my patient. Even where improvement occasionally follows upon the use of any of our stock medicaments, that improvement can rarely be predicated with confidence; nor reliance felt that the same remedy will arrest or "stamp out" the disease in its inceptive stage, on the occasion of its return. With a return of the disease, a return to a round of the old empirical routine is almost a necessity.

Of the common notions held by even the best informed regarding psoriasis, I will relate an illustration which indeed has evoked the present communication. About eighteen years

ago I consulted with the late Dr. Macfarlane, Professor of Medicine in Glasgow University, upon the case of Mr. G., a gentleman aged 24, who for nine years previously had suffered under a bad form of psoriasis. It was not a *pro-formâ* consultation, for I had exhausted my resources and really wanted help. But Dr. Macfarlane, after learning the course of the ailment and of its treatment, frankly stated that he could only sympathize with me in my difficulty, nor would he make any other suggestion than that of beginning *de novo* a round of the old remedies in the hope that one or other of them might occasionally mitigate the severity of the affection. "But depend upon it," said he, "that man's blood has got a taint, his constitution has got a kind of twist, and his disease will never leave him, do what you may."

During the eighteen years that have passed since my consultation with Dr. Macfarlane, this case of Mr. G. has been continuously under my observation, and my efforts for its cure or relief have been stimulated by the circumstance that my patient is also a much valued friend. In addition to my own promptings, the suggestion of several eminent specialists have been carried out. There has been careful regulation of diet, habitual exercise in open air—whether in walking, riding, or yachting—changes of climate at home and abroad, &c., together with potent constitutional remedies and local applications, all exhaustively employed. As is common in such cases there have been from time to time occasional gleams of improvement, but never of perfect relief, and his condition on the whole has been so distressing that he shunned society and ofttimes "felt weary of his life." In September last the disease was at the worst that I can recollect, and from his head to his feet the eruption was so copious that there existed only here and there some straggling patches of sound skin. Meanwhile, setting aside the mental disquiet and bodily discomfort, he remained of robust habit, and his general health as regards function was good. Such was the state of matters at the end of twenty-seven years of customary orthodox treatment.

About this time, from various indications, I judged that in advance of most modern authorities, Dr. Balmanno Squire, of London, had given evidence of being a man of progress, and of original conception in his own line of a skin specialist. I therefore, although with much misgiving of any profitable result, recommended my patient to place himself under the care of that gentleman, provided that, after full knowledge of the history of the case, he had reasonable hope of giving relief through some mode of treatment differing from what had so long and so fruitlessly been employed.

Dr. Squire, after seeing my patient and hearing all necessary

details, expressed his conviction that, unless in a remedy which had recently come within his knowledge, the remarkable properties of which in this disease he had discovered, and in great measure already verified, he would really have had nothing reliable with which to cap the previous treatment. But with considerable consideration for the valuable time and other interests of my patient, he resolved to employ double weapons, and to conjoin the use of an internal or constitutional remedy, viz., phosphorus in large doses. But he relied mainly on the new remedy, chrysophanic acid applied locally in the form of an ointment. He began treatment on 24th of September 1877 with an industrious energy that my patient felt to be "tremendous," and by 1st October the greater part of the eruption had disappeared. On 26th October my patient left London for Glasgow in a condition of complete freedom from his long-experienced affliction. I saw him on the day after his return, and had him stripped and overhauled, my expressions of surprise and admiration evidently giving intense delight. No transformation from extremest disease to the most perfect health could be more marvellous—no illustration of medical skill more creditable, and no instance of relief from suffering through the use of medicaments more complete. The cure was a perfect work of art, and my congratulations to my patient and acknowledgments to Dr. Squire were sincere and heartily expressed. The cure continues complete at the present date—an interval of six months.

[Dr. Adams here relates 10 cases in which this treatment was more or less successful, and then adds]

So much for the curative results of this novel remedy, the history of which now claims a short notice. About three years ago Dr. Fayrer, of Calcutta (now Sir Joseph), in a letter to the Medical Times and Gazette, directed attention to a secret remedy called Goa-powder, sold by the chemists of Calcutta and Bombay, and used by the natives for the cure of herpes circinnatus, chloasma (pityriasis versicolor), and intertrigo, and which he had found speedily and certainly effective—in both respects far beyond any other remedy known to him. This letter elicited, a few months afterwards, a thoroughly well-informed communication from Dr. Lima, of Brazil, who, besides corroborating the therapeutic virtues of the remedy, traced the material from its place of production to its place of sale. Dr. Lima showed that this substance had for a long period been exported from the province of Bahia, formerly a Portuguese colony, and from thence to Goa in Hindustan, then and now a Portuguese possession. From Goa it is commercially distributed throughout India. In Brazil it is called araroba powder, signifying tawny-

coloured. Under the several names of Goa-powder, Bahia powder, and araroba powder, it has long been largely employed in South America, India, China, and the Malay Archipelago for the treatment of parasitic diseases of the skin, particularly ringworm. But until the recent researches of Dr. Balmanno Squire it has not, so far as I am aware, been used in Europe, and the first recognition and demonstration of its extraordinary properties in this country are fairly due to this gentleman. Goa-powder is derived from the medulla or pith of the stem and branches of a tree belonging to the leguminosæ, there being several species of the araroba, which, like the well-known Brazilwood are employed for dyeing purposes. The powder, while sold as a secret remedy, fetched a high price, being dispensed in Bombay at 4s. 6d. per ounce, and, a little more than a year ago, when brought to London as a curiosity, its retail price was 10s. an ounce.

Dr. Squire had his attention directed to the powder about eighteen months ago by a gentleman affected with psoriasis, who stated that he had often when in China cured himself of a patch here and there through the use of a powder employed in that country for the treatment of ringworm. This gentleman demonstrated upon himself the effects of the powder with a result so surprising that Dr. Squire's interest was strongly aroused, and he immediately investigated the character and action of the remedy. He was soon convinced of the thorough efficiency of the drug in a field as yet unexplored by any other observer, and, after testing separately the various chemical constituents, he ascertained conclusively that all the healing virtues belonged to one of these, viz., chrysophanic acid.

Chrysophanic acid is treated of very shortly in modern works on chemistry, but its first recognition as an article of the materia medica is, so far as I am aware, to be found in a note attached to the latest edition of Squire's Supplement for the British Pharmacopœia. Its therapeutic qualities as the active agent in the treatment of skin diseases are certainly noticed for the first time in the British Medical Journal of November, 1876, by Dr. B. Squire. That gentleman has, on several occasions since, strongly expressed his opinion of its remarkable qualities, and in February last he embodied in a pamphlet of 100 pages (Churchill) the more notable facts of the history of the drug, together with his experience of its effects, and his recommendation as to the best mode of its employment. To that monograph I refer for fuller details on some points that I am treating of only in a discursive manner.

Chrysophanic acid exists as a more or less important constituent of several vegetable substances. It has been found in the common dock-lichen, in the cathartin of senna, and in the

medullary rays of Indian rhubarb, being known in the latter case under the names *rhœin* or *rheic* acid. But for commercial purposes it has as yet only been obtained from Goa-powder. Dr. Attfield, in a very careful investigation, has shown that chrysophanic acid forms upwards of eighty per cent. of the Goa-powder, and he conclusively demonstrated the chemical identity of the Goa, the Bahia, and the araroba powder.

The high price of chrysophanic acid constituted a great hindrance to the employment of the drug, but within the last twelve months the cost has fallen from 10s. to 5s. per oz., and the probability is that, with increasing demand, the cost will be still further reduced. Dr. Squire recommends that chrysophanic acid should be used in the form of ointment, for which the following formula gives such a full strength preparation as I have accustomed myself to employ: ℞. Chrysophanic acid ʒ ij., lard ℥ j. Digest the acid in the lard at the temperature of boiling water for half an hour, stirring constantly. When "set," mix with pestle and mortar.

This strength of ointment will be found generally suitable, but it may, of course, be varied, and reduced in strength to suit cases where the skin is extremely sensitive. To prevent disagreeable results from over-action of the remedy, as well as to ensure that it is not employed in a careless or half-hearted manner, I recommend that personal attention of the medical man should be given during the first week of treatment, and that the case should be seen from time to time while the treatment is continued. The details, if carefully supervised in the early stage, will ensure a progress within a few days that otherwise will not be made in several weeks. These details, together with the accidents of treatment and the *modus operandi* of the remedy, will be best understood by portraying a typical case. The patches of psoriasis are first rubbed well with a piece of stout blotting-paper or common rag, moistened with benzole, the object of which is to absolve and remove that natural greasiness of the skin which, like a film, opposes the ready penetration of the skin by the specific. I believe that benzole —a tar product—has also some specific action, and is therefore otherwise a useful adjunct. This preliminary application of benzole facilitates the next application, viz., hot water freely used with soap, the object being that of softening the layers and fragments of epidermic scales. The removal of the scales is a matter of prime importance, and must be effected whether by scraping with a blunt knife, or by scratching and picking with the finger-nails. It is only when the film of epidermic scales are removed that the skin proper can be fairly attacked, and I have not been deterred by the starting of a little blood from pushing to an extreme degree this scraping and scratching

operation. The ointment is now rubbed in very perseveringly for several minutes at a time with the tips of the fingers. As the ointment is a dye, and will stain for some days the fingers and nails, I occasionally prevent that disagreeable accident by recommending the use of indiarubber finger-stools such as are frequently employed by photograpers. These three stages of local treatment, viz., the wiping with benzole to remove grease, the thorough soaking of thickened patches of skin with hot fomentations, washing with soap, diligent scraping away of epidermic *debris*, and the patient rubbing in of the ointment, may be employed three times a day if the patient can command the necessary time and facilities. There is no question that the more frequent the application the more rapidly does the disease come under the action of the curative agent. But under ordinary circumstances a morning and evening application seem to give a sufficiently satisfactory result.

Within two or three days there comes on a dusky erythematous redness of the skin, partly the effects of the dye and partly the stimulant and irritant effect of the drug. As its action increases there occurs an inflammatory thickening of the skin in some cases and in particular situations, causing œdema. Thus, the face becomes puffy and the eyelids tumid. It also occasionally happens that stiffness from distension of the skin, with smart sensations of burning and tingling, will be felt all over the affected surface, but seldom to that extent to be positively distressing. In two instances I have known several boils of trifling severity to be produced on the dorsal aspect of the body, where the skin is naturally coarse, and prone to take on over-action; and in the case of Mr. G. I learn that he experienced a full crop of boils, and was, he says, for several days and nights "kept in a very lively condition." Where the hair of the scalp is white or light gray, there will occur a phenomenon against which it is well to prepare the patient. In the case of my friend Mr. G., whose hair is naturally white, it rapidly assumed "a fine purple" colour, ending in "a bright canary," and in the change passing through shades and gradations that would excite the envy and aspirations of a decorative house-painter. Indeed he found himself so much "the admired of all observers," and so much the source of wit to the London street *gamins*, that he could only venture a "constitutional" walk after nightfall, while his day life for several weeks was passed in a Hansom-cab, as he travelled to and from his numerous appointments with Dr. S. I have not, however, observed this unpleasant accompaniment of the treatment to have occurred in other cases where the hair was of the darker shades. But it is satisfactory to know that, however the hair or skin or nails may be stained, in every instance the natural

colour is soon restored after discontinuing the treatment—often within a few days. This restoration does not occur, however, in the case of the underclothing, sheets, &c., when soiled by the ointment, and the patient should be warned, that only articles that are of little value, or in which colour is of little moment, should be exposed to contact with the remedy. One farther warning exhausts all that need be mentioned. A properly secured nightcap will prevent the pillow from getting smeared, and a possible transfer of the substance to the eyelids, which would in such a case certainly become swollen. And if the patient is in the practice of rubbing his eyes awake at early morning, he will do well to sleep in gloves.

After a few days' active use of the ointment the diseased patches of skin cease to shed scales—a few days more, and they become smooth and polished, surpassing in these conditions the surrounding healthy integument—and still a few days more, and then a remarkable change occurs. The affected spots become white—almost unnaturally white, in this respect contrasting very markedly with the surrounding healthy skin, which meanwhile retains the dingy nut-brown staining effect of the dye. If there is now no rough marginal line indicating the original outline of the diseased patch, and if no more scales are being shed, or capable of being picked off or scratched away, then the application of the ointment may be discontinued. Two or three applications of the benzole and of soap and water will within a few days leave the skin free from any trace of the disease. It becomes, indeed, impossible to trace the outlines of where the patches of psoriasis were situated, so completely does the skin resume its original integrity of aspect and of function.

And here there would seem to be an interesting question opened for consideration, but to which I will now do little more than make a reference, and that is the *modus operandi* of the remedy. Dr. Squire has some shrewd observations on this point. Most irritants of the skin have very different kinds of effects. Thus the vesicle or pustule of croton oil and tartar emetic, the vesication or erythematous irritation of cantharides and mustard, although differing so widely in their special irritation, seem all to act chiefly on the surface only of the skin proper, and do not affect the deeper structures. But the action of chrysophanic acid penetrates deeply, and affects the subcutaneous areolar tissue with less of surface irritation than these or most other rubefacients. The "quality" of the irritation it produces seems to be different, and to be specific. It seems to affect more or less the entire thickness of the skin, in this respect contrasting very materially with the ordinary local remedies for skin diseases, which have for the most part a very

superficial action. May not a full consideration of its action in ringworm and other parasitic diseases lead to the conclusion that psoriasis—like pityriasis versicolor, so long and so lately held to be constitutional—should also be classed among parasitic affections of the skin?

And now comes another important question. Is the cure which I have shown to be so speedily and so certainly effected a permanent cure? The time which has elapsed is too short to admit of answering in the affirmative. But it is scarcely reasonable to expect as much, or to suppose that the same conditions which originated the ailment may not again combine. It ought to be sufficiently satisfactory if the remedy is as reliable as I have shown it to be. And if it will arrest and stamp out a relapse, then its importance and value remain as great as can fairly be hoped for. I think that two of my cases will throw some light on this latter point. In one case, that of Master C., there was, as I have already stated, an element of disappointment. On 10th December I thought him completely cured, and discontinued treatment. I did not re-examine him until 1st February, when I was startled by detecting on close inspection a number of suspicious white spots like an efflorescence, consisting of epidermic scales. They were of size varying from a pin-head to a mustard-seed. Being uncertain how far they indicated a reaction from previous irritation resulting from strong applications, or a relapse, I did nothing for a week, when I found that they had increased in size, being in some instances of the diameter of a pea. They resembled dried spatterings of lime, and when scraped the skin was found red underneath. There was very clearly a relapse. I now resolved on an experiment I had for some time contemplated, and applied to the right arm and the same side of trunk a solution of chrysophanic acid in liquor potassæ, painting each spot with a camel-hair pencil. To the other side of trunk and the other arm I applied—for reasons to be afterwards given—a watery paste of alizarin, causing it to be thoroughly well rubbed into the skin. Within a week the specks of incipient psoriasis had entirely disappeared from both sides of body and from both arms *cum passibus æquis*. I have caused this patient to call upon me at weekly intervals since, and I have kept careful watch for the return of suspicious spots. A very few have appeared on the arms—none on the legs—and under the application of alizarin these spots have promptly given way. In this case, therefore, under two remedies—as I think of allied nature—an impending relapse has been promptly stamped out. Whether this result has been a coincidence or a consequence I will not at present say.

In another case, that of Miss E., there have appeared on the

arms three small spots, each of the diameter of a pea. To these, as in isolated patches in a few other cases, I have applied chrysophanic acid mixed with collodion in the proportions of 20 grains of the first to a quarter of an ounce of the last. The spots are well rubbed with hot water and soap, then with strong alcohol, and lastly, painted with the collodion varnish, which instantly dries. At an interval of eight or ten hours the crust of varnish, which adheres very firmly, is rubbed briskly, and removed with a rag dipped in strong alcohol, after which a fresh coating of the varnish is applied. The patches so treated have undergone the same characteristic changes which follow the use of the remedy in the form of ointment, and little more than eight or ten days have sufficed for their removal. In this case of Miss E., a relapse, to all seeming, has been also promptly stamped out by recourse to the original remedy, although in a modified form of application.

The object of using in one case a solution of chrysophanic acid in liquor potassæ, and in another of a chrysophanic acid varnish, was to obviate the greatest drawback to an invaluable remedy—the only real obstacle, indeed, that may occasionally prevent it from being pushed to its utmost efficiency. While hopeful of relief from a great evil, there is a ready submission on the part of the sufferer to the disagreeable incidents which I have referred to as liable to occur in the course of treatment. But when the ailment has been substantially removed, and the patient has fairly got back to the comfort of clean linen, he naturally shrinks from returning to a practice which so surely disfigures his underclothing with ugly yellow stains, and frequently destroys them for farther use. He is, therefore, apt to submit to the insidious approach of a few spots in the hope that they will spontaneously subside—a hope no sensible medical man will encourage. It was, therefore, to me a desideratum to contrive such a modification in the manner of applying the remedy as will, on account of its cleanliness and the ease of its application, encourage the patient to resort to it without loss of time. Hitherto I have found this collodion varnish to answer admirably my intention, and have had no apologies to make for injured shirt-sleeves, cuffs, or neck-collars, and I will employ it more frequently in future, particularly in cases where the patches are few, the situations exposed, or the skin so sensitive as to suffer much from the active frictions which are necessary in the right application of the ointment.

In the pursuit of a hypothesis I have employed alizarin in two cases—first in that of Master C., as already described, and next in that of Master L. It will be recollected that in the latter case there existed a coarse thick patch about 2½ inches long on the front of left thigh which was not treated with

chrysophanic acid. This was the worst patch of the three, and it was of the longest duration. To it I applied an ointment of alizarin made up in the strength of ʒ ij. to the ounce of lard, and used precisely under the same conditions as the chrysophanic-acid ointment. The treatment of this patch was commenced on 24th March, or about three weeks earlier than was pursued in the other patches. The effects were by no means so speedily apparent as is customary when chrysophanic acid is used, but the result has been equally effective, equally decided. There was less irritation under the use of alizarin—less intumescence of the integument, but there was much the same discolouring of the skin, and much the same white and and smooth aspect of the patch as it emerged from the dusky red surface into a state of complete renovation. Was it the alizarin, or was it the combined action of benzole, of soap, and of hot water that effected the cure of this thick coarse patch of psoriasis? I am unwilling to dogmatize on a single case, and quite willing to admit that it gives no sufficient data for a decided opinion, but I may observe that it is corroborated by that of Master C., where a decided relapse affecting both arms and trunk of body was effectually arrested, and the disease removed from one side of body and from one arm, under the use of alizarin only, no benzole or other possible specific adjunct being employed upon that side. While, however, I am not more sanguine than such limited experience with alizarin should justify, I am nevertheless hopeful that the idea which I am now suggesting will lead directly or indirectly to a good practical result. For I am inclined to think that there is an analogy in the properties of alizarin and chrysophanic acid. So far they seem to be therapeutically isomeric, and my mind has been all the more inclined to admit the resemblance, because I have ascertained that they are chemically isomeric. I anticipate as possible and probable that alizarin may either prove a cheap substitute for chrysophanic acid, or that it may form a basis or a link in the artificial production of chrysophanic acid at a cheap cost.

It may help others to a conclusion if I exhibit the lines on which my mind has worked in the investigation of this problem. When I made a saturated solution of chrysophanic and in liquor potassæ, viz., ʒ ss. to ʒ ij., I observed during, and even after, the reaction a distinct odour such as characterizes some of the coal-tar products. I immediately connected this with the generally admitted therapeutic virtues of tar in skin diseases. It then occurred to me as possible that chrysophanic acid—the present wholesale cost of which is 80s. per lb.—might be manufactured cheaply from the coal-tar products. To this belief I was led by what I already know of the striking results

of modern chemistry, which, when directed to these products, have produced the aniline colours, together with anthracene and alizarine. Alizarine, one of the organic vegetable pigments, was originally derived from madder, and cost 14s. per lb. It is now made from the coal-tar product anthracene at 1s. per lb., and the artificial preparation is in every essential of elementary composition, and of characteristic quality, identical with the original preparation derived from madder. Now there are several striking characteristics of alizarin which compare harmoniously with those of chrysophanic acid. Alizarine is also an acid—a weak acid, it is true, but quite entitled to be called alizarinic acid. Alizarine is convertible into anthracene when heated with zinc dust, and conversely anthracene, by another process, is convertible into alizarine. Chrysophanic acid is also convertible into anthracene when heated with zinc dust, but the process has yet to be devised, as I think it will be, that will convert anthracene, or alizarine into chrysophanic acid. Dissolved in potash, chrysophanic acid produes a fine deep purple or violet colour, identical with the reaction of alizarine and potash. And not to exhaust similitudes, the elementary formula for alizarine is identically that attributed to chrysophanic acid.

It was only through demand, and stimulated by competition, that ultimately, and after long years, the process was devised and applied, by means of which alizarine is now artificially prepared, and its cost reduced from 14s. to 1s. per lb. It seems to me that demand will in time develop a process for the production of chrysophanic acid at a cost that will bring the remedy within the means of many individuals, and of various public institutions, where the present high price is a serious obstacle to its use.—*Edinburgh Medical Journal, July* 1878, *p.* 16.

47.—TREATMENT OF ULCERS AND VARICOSE VEINS BY MARTIN'S STRONG ELASTIC BANDAGE.

By GEORGE W. CALLENDER, Esq., F.R.S., Surgeon to St. Bartholomew's Hospital.

Having for some time used the elastic bandage as recommended by Dr. Martin, of Boston, U.S., for the treatment of varicose veins and ulcers, it is right to express my opinion that his treatment is practically good. Under its influence, ulcers not of a specific character do certainly heal, and quickly; and as this is effected whilst the patient follows his usual occupation, the merit of the treatment is evident. Although there has been scarcely time since I received the bandages from America for use in St. Bartholomew's Hospital to test their efficacy in the continued treatment of varicose veins, yet I have found that

the application of the bandage is at once followed by a sense of great relief. A man of thirty-six years of age, who had suffered for eleven years from varix of either internal saphenous vein, extending as high as the groin, experienced at once and since greater relief from the use of the bandage than he had ever had from silk elastic stockings, although his position was such as to allow of his wearing the best which could be made for him. I have used the bandages for many cases of ulcer of the leg, chiefly due to varicose veins, and always with the good result stated by Dr. Martin, with whose practical statement respecting the treatment of these ulcers I agree.

There are some points named by Dr. Martin which are essential if good results are to be had. The first of these is as to the quality of the bandage itself. It should be just such as the author describes, and as is supplied from America. If thicker, it would be clumsy; and if thinner, it needs to be more tightly drawn to offer the due resistance, and the edges are more apt to indent or cut into the flesh. If Dr. Martin's treatment is to be tried, the bandages should be such as his experience has led him to prefer.

The bandage should be applied before the patient rises from his bed in the morning—before, that is, the veins of the leg become distended by the impeded column of blood within them. It should be applied with just snugness enough not to slip down. The moment the foot is put to the ground the limb is so increased in bulk by the increase of blood in the veins that the bandage becomes of precisely the proper degree of tightness, and, no matter how active the exercise or labour of the patient, it will remain in position all day. When the patient undresses at night the bandage is to be removed, and the limb wiped dry, a piece of soft old linen moistened with oil, or some equally simple dressing, laid on the ulcer and retained in place by a few turns of an ordinary roller. The bandage should be sponged with water (cold will do, but warm is better), and hung over a line to dry in readiness for the morning, or it can be wiped dry at once, or rolled up with the tapes in the centre. Such is the dressing for the night; in the morning the leg can be washed, but, whether it is or not, all traces of oil or cerate should be carefully wiped away, as contact with the bandage of any fatty matter would tend gradually to injure the rubber. This is the whole treatment. Rubber bandage all day, with erect position and exercise. The simplest possible dressing (merely to protect the ulcer from injury), with the horizontal position and rest, all night. Any pimples which may form are left to be treated by the rubber, and if the skin become chafed a light covering, as of bunting, is applied as a bandage under the rubber, and the same is recommended to absorb excessive moisture.

No more distressing cases than of patients suffering from chronic ulcers crowd our hospitals, and I must say that I feel much indebted to Dr. Martin for the suggestion he has published, and which he has made practical application of for many years with, as he says, great success. My present but recent experience leads me to believe that he is to be confirmed in the statement he makes, and which any who like can read, as it is very clearly and ably put in the Transactions of the American Medical Association for 1877.—*Lancet, Oct.* 12, 1878, *p.* 503.

48.—ON VARICOSE ULCER AND ITS TREATMENT.

By JOHN GAY, Esq., Senior Surgeon to the Great Northern Hospital.

A general impression prevails that an ulcer on a limb, the veins of which are either simply tortuous and enlarged, or actually varicose, is related to such veins as effect to cause; and many a vein has been needlessly mutilated through this misapprehension. Moreover, in order to bring about a reconciliation of this view with the conflicting testimony of results, ingenuity has been rife in devising various modes of treating such veins—caustics, ligature, extirpation, &c.—as though, in despair of a principle, a special charm was to be discovered in some particular mode by which their destruction is to be accomplished.

I believe we must seek for other principles in order to establish our claim to having satisfactorily dealt with the question as to varicosity and its bearing on the process of allied ulceration.

A case is now under my care in the Great Northern Hospital, which I will briefly relate, together with the treatment and the reasons for its adoption.

Mrs. W., aged fifty-eight, had a small but very painful ulcer over the saphena vein on the lower third and inner side of the leg. The skin around it was puckered and eczematous, and figured with the ordinary dermoid form of varicosity. This extended to various parts of the limb, and over the inner half of the dorsum and the side of the foot. From the upper edge of the ulcer a branch of the saphena, a constant tributary, for many years varicose in the highest degree, passed for the distance of about an inch and a half, and then lost itself in its trunk. It could not be traced beneath the ulcer surface. There was a varix in the upper part of the leg, and about midway between it and the ulcer there was another visible portion of vein extremely varicose.

The woman had often laid up, and tried various expedients, but with only temporary success; and she was now unable to

stand on the limb but for a very short period without great pain. On lifting the foot above the level of the trunk the vein was immediately emptied of its blood, and it filled again on its being placed firmly on the ground. It *filled, however, from above, and not from below.* I tied the vein simply with a piece of ordinary ligature silk, passing it with a common curved needle under the vein, and including as little of the skin as possible, just above the ulcer and below the varix; and, as the skin was puckered in around the ulcer, I scored it on both sides. The ligatures were removed after the course of two or three days, when the vein was found to have been obliterated in both places.

All ulcers with varicose veins are not varicose ulcers. Varicosity, in order to constitute a cause of ulcer, must be seated in certain veins, and be attended with certain disturbances of the circulation; that is, such a vein must be useless as a channel in aiding the onward flow of the blood by its own natural elasticity or muscular forces. A vein simply enlarged, thickened, and tortuous, will continue to be possessed of these qualities, although in diminished intensity, and therefore useful. The course of a vein to extreme varicosity is through *this*, the earliest stage—that of hypertrophy—onwards through atrophy of its inner tunics; morbid, sometimes caseous, deposits; degeneration; softening; fragmentary dislodgment of muscular elements; localisation and valvular incompetence. I have often, by the aid of the microscope, seen shreds of elastic and contractile tissue in course of being detached from the wasted and ampullary outer tissue, and have reason to believe that these shreds when detached are not less likely than clots of blood to become the source of remote embolism. The vein may thus become comparatively useless as a channel for the passage of the blood; but with all this it may not be a *cause* of ulcer. We must go a little further for the explanation of so related a coincidence.

Now varicose ulcers have certain favourite sites, and these are in relation to *certain valvular arrangements* to which I shall now direct attention. They are met with principally in the lower third of the leg, and below the ankle-joint, either on the outer or inner side of the foot. As I showed in some papers which appeared in the Lancet, the leg and foot are segmentally divided in fixed situations by a series of valve barriers, which extend horizontally across the limb, and intercept all regurgitation of blood or any attempt to force the finest injection from a segment above, through one such barrier, to a segment below. One is met with a short distance below the knee-joint; a second a short distance (about a third of the length of the leg) above the ankle-joint; a third immediately below that joint; and there are others still more distally

placed. A varicose ulcer is met with in close contiguity to one of these barriers, especially those above and below the ankle-joint, and is closely associated with incompetency in one of the valves of such barrier belonging to a saphenous tributary—not the saphena itself—closely adjoining. The explanation is this: During the circulation in its normal state the valves do not, as alleged, contribute to maintain the onward current by supporting the columns of blood in advance of them; the cusps during this process are flattened against the sides of the veins, and are only subjected to closure, and that temporarily, for the purpose of adjusting the blood-tension throughout the veins constantly in peril of disturbance. Aided by certain plexiform arrangements, *this* is the main purpose of the valve system. It can only, therefore, be when a vein is diseased to the extent that it is simply passive in relation to its current, as attested by the behaviour of the blood on alternately raising and depressing the limb, and, at the same time, its *valve—always a barrier valve*—is rendered incompetent, that its part in the composite function of regulating blood-tension is suspended. The result is that in this especial vein, the weight of the superincumbent blood-column is in excess of the forces engaged in propelling it; the skin bordering and intermediate between it and the valve barrier next below becomes congested, and ultimately ulcerates. The resultant sore is a genuine "varicose ulcer." If the tissues be relieved of their superfluous blood by rest and leg elevation, the sore will heal; but it will repeat on adopting an opposite course. The only effective remedy consists in the establishment of a quasi-valve in that part of the vein in which it has become inert; and this can only be done by some one of the ordinary means of effecting its obliteration. Thus the essential factors in respect of a *varicose* ulcer are extreme vein degeneration, and resultant incompetency of its valve—always a *barrier* valve; the cure, its substitution by obliteration of the vein-channel in that situation.

The foregoing views are inferentially derived from a study of the veins of the lower limb after repeated retrograde injection, and their application to the facts of varicose disease as elicited by close clinical observation.—*Lancet, June* 29, 1878, *p.* 928.

49.—TREATMENT OF ACNE ROSACEA.

By JAMES STARTIN, Esq., F.R.C.S., London.

Authors and writers from the earliest ages up to the present day seem to have considered the successful treatment of acne, particularly that form known as acne rosacea, to be of some considerable difficulty. I have been much rewarded lately in hospital and private practice with some very good results by

the carrying out of the following mode of treatment. In the first place, vegetable acids, mineral waters, purgatives, arsenical preparations, mercurials, and other specifics, are not indicated in this eruption of the skin, nor have I ever found them of much avail. The diet should be carefully regulated, all indigestible food avoided, and little or no stimulant should be allowed, except in those cases where the powers of digestion may require slight stimulation or excitation to perform their proper function, and the discriminate use of chalybeates, combined with mineral acids, vegetable bitters, and tincture of iodine, I consider to be our most useful internal remedies. These are considerably augmented by the sulphur vapour douche or bath. But it is the external treatment of acne that is the most important, and it is to this that I should wish to draw more particular attention. If at the onset of the affection the sebaceous glands and follicles become affected and overloaded, they should be relieved by pressure between the finger and thumb-nail or a large watch-key, and by frequent washings with warm water and oatmeal, after which a good rubbing with a flesh-brush will remove the contents of a number of the pimples. If the disease is more advanced, and the indurated spots become enlarged and painful, the vapour douche is then of much service, followed by an ointment of the ammonio-chloride of mercury (thirty grains to the ounce, and camphor (one ounce), or the gentle application of iodide-of-sulphur ointment at night, and the use of a cooling zinc or calamine lotion and glycerine (of the former about two drachms of each to the ounce), to be painted over the part affected two or three times a day. With the judicious use of the above remedies and applications we may probably remove and cure the disease; but troublesome redness, with often great vascularity of surface, still remain in spite of all treatment, and it is to this stage of the affection these remarks tend. To these small indurations and superficial venous enlargements of texture I apply the strong acid nitrate of mercury with care by means of a spun-glass brush. One or two applications, with the immediate use of blotting paper, will usually suffice to effect complete obliteration of the veins without a scar, and I find after a few days the morbid state of the capillaries has undergone a complete change for the better. Nevertheless, we may still have some of the larger capillaries, which keep up the localised redness and induration, to contend against; then the mercurial acid treatment does not hold good. I therefore divide each engorged superficial and conspicuous vessel with the point of a lancet, and, should the hemorrhage be much, I apply a small ring of silver or steel, about the eighth of an inch in diameter, which I have fixed into a handle at right angles, over the divided ends of the vessel. I then insert

a minute grain of nitrate of silver upon the end of a probe, which at once stops the bleeding, and soon obliterates the engorged vein, leaving only a small, black, discoloured spot, which may easily be removed by the application of a solution of iodide of potassium.

The above treatment may appear somewhat heroic for so common a skin affection; but I can only state I have made use of it in a large number of cases in hospital and private practice, and find it a most successful mode of treatment.—*Lancet, June 22, 1878, p. 925.*

50.—ON THE TREATMENT OF WOUNDS.

By GEORGE W. CALLENDER, Esq., F.R.S., Surgeon to St. Bartholomew's Hospital.

From time to time, I have stated the results obtained with the cases under my own observation; and as these results continue their satisfactory character, it is unnecessary to refer to them further. It may, however, be of interest if it be mentioned that, tabulating the cases treated in the two wards which are under my care (one for male, and the other for female patients), there have been under treatment during the last eight years 2,070 cases, with 68 deaths; showing a mortality of about 34 per 1,000, or of 3.4 per 100.

Amongst these cases are included the chief operations which have been, of necessity, performed. I say, of necessity, because I trust that each year we are learning how better to avoid the need for having recourse to many an operation by closer attention to details of treatment, more especially in the case of severe lacerated and other wounds. Not that the total number of operations is thus materially affected, for, with improved methods for the treatment of wounds, operations are now indicated which, until recently, were rarely if ever practised.

The death-list is largely made up of patients dying from burns, from diabetes, albuminuria, phthisis, caries of the spine, spina bifida, hernia, and such various troubles as, unfortunately, are still prominent in such a list.

Granting, with Professor Tyndall, that the method we adopt is akin practically to that used elsewhere, I wish more particularly to point out, as I have before stated more than once, that it is essentially one of extreme simplicity, one which can be effectually carried out by any surgeon under any circumstances without any difficulty.

In illustration, the case of a patient aged 16 may be referred to. For the treatment of a severe case of genu valgum, I opened the knee-joint, and with a saw separated the internal condyle of the femur from the shaft of the bone. The condyle being dis-

placed upwards, the limb was easily brought into the straight position, and in that position was secured by a long splint (Dr. Ogston's operation). The wound, which admitted at one and the same time the long knife used for penetrating and dividing the soft parts and a stout saw, had a depth of about three inches and, where the skin was divided, a length of about one inch. No carbolic or other spray was used at the time of the operation or at any time subsequently. The wound was covered with a doubly folded piece of lint soaked in carbolised oil (1 in 12); over this was placed a larger piece of lint, likewise soaked in the oil; and over all a yet larger surface was covered with some ordinary gutta-percha tissue, The dressings were examined daily, the gutta-percha tissue being lifted and the outer layer of lint being changed, the gutta-percha being then replaced. The inner layer of lint was left undisturbed on the wound for three or four days, being each day refreshed with carbolised oil (poured upon it or painted over it with a camel-hair brush). Whenever this layer of lint was changed, the wound was freely exposed to the air. The patient got well without any constitutional disturbance, and the parts healed in the ordinary course. Treated in a similar manner, there are at this time under observation in one of my wards a severe compound fracture of the femur into the knee-joint, and two cases of compound fracture of the leg; in one of the latter, it was not necessary to disturb the inner layer of lint for nine days after its first application.

Of course, these are but casual illustrations, from many similar cases, to show how a very simple plan suffices to ensure that condition of cleanliness which is essential to the sure and rapid healing of severe wounds, or wounds practised upon parts which it was formerly considered hazardous to involve in operations. With this cleanliness must go "rest," and with this will be ensured "ease;" and when deep wounds, or wounds the surfaces of which are not easily and closely adapted, are concerned, thorough drainage is of prime importance.

I do not like to write of "antiseptic" treatment, because I regard the treatment of wounds as a matter of strict adherence to the established rules of surgery, and I consider it undesirable to prefix a term which may seem to specialise the method of wound-management. But the results claimed for antiseptic surgery can be ensured by the plan which we have now used for many years, and of which I venture to write as being as simple as it is efficacious.

I believe, with the exercise of care and by close attention on the part of the surgeon, that different methods of treatment will yield good results. As to the plan employed in my wards in relation to the proposition that germs are causative in pro-

ducing bad results in the case of operation and other wounds (but without committing myself to this view of their influence), let me add that, whilst other methods aim, amongst other things, at the entire exclusion of such germs from wounds, I shall rest content to allow their ingress, feeling quite sure that after such entry they are absolutely prevented from doing any harm by the treatment we adopt.—*British Medical Journal, May* 11, 1878, *p.* 673.

51.—UNIVERSAL TALIPES-SHOE.

By H. A. REEVES, Esq., Surgeon, Royal Orthopædic Hospital.

The accompanying woodcuts will give an idea of a simple and inexpensive apparatus which the ingenious and punctual orthopædic mechanist, Mr. Schramm, of 64, Belmont Street, Chalk Farm-road, made for me some months ago. Its object,

which is fully attained, is to do away with the frequent changes of shape and size of the splints and shoes generally used, and to save time, trouble, and expense. On the outer side there is, as usual, a metallic stem connecting the leg and foot pieces; but the joint is a modified ball and socket, and is perforated by a screw, to which a ring is attached (a bar may be substituted as perhaps giving greater power for screwing up, and as less likely to hurt the fingers), by loosening which the shoe can be set at any angle, and fixed by reversing the action of the screw. The foot is fastened to the foot-piece, and inversion or eversion, &c.,

prevented by passing the bandage over and under the foot in the desired direction. For severe cases a spring bar can be attached to the outer or inner side, as the case requires. After tenotomy (if this be needed), the foot can be put up in the deformed position, and brought quite straight, gradually or quickly, by the use of the modified ball-and-socket joint. The instrument may be made of various sizes and of any strength, and as it is efficacious, simple, and cheap (from 7s. 6d. to 10s. for hospitals), and as nothing else is needed till the patient is ready for the boot and support, I need not say more to show its value.

I never was in favour of using complicated apparatus in any department of surgery, and as I have had the opportunity of treating a large number of deformities of various kinds—most of them severe cases—and have always made successful use of simple methods, I cast about me for a simple shoe which would do for any kind of talipes. I believe I have found one, and it is pleasant for me to be able to add that my colleagues at the Royal Orthopædic Hospital, Messrs. Read, Balkwill, and Baker, have expressed themselves as much pleased with the shoe. Mr. Brodhurst and his coadjutors at that institution always adopt simple methods and apparatus; and, provided an operation or an instrument be efficacious, the simpler it is the better is it.—*Medical Times and Gazette, Oct.* 12, 1878, *p.* 434.

SYPHILITIC AFFECTIONS.

ON THE MODES OF ADMINISTERING MERCURY IN SYPHILIS.

By JOHN DUNCAN, Surgeon to the Royal Infirmary, Edinburgh.

There are four principal methods by which mercury may be administered—by subcutaneous injection, by fumigation, by inunction, and by the mouth.

About two years ago I read a short note before the Medico-Chirurgical Society concerning some experiments on the subcutaneous injection of mercury. Since that time a second series of observations has been made, and with results which have somewhat modified the opinion then expressed.

On both occasions the experiments were conducted with the bicyanide, the biniodide, and the albuminous solution of the bichloride of mercury, and in the latter series with the bichloride in simple watery solution, in solution from Savory and Moore's lamels, and in combination with morphia. Very little variation was made in the amount injected at one time. A few injections of one-twelfth of a grain were tried, and it was found that the amount made very little material difference in

the local irritation. It may be regarded, therefore, as likely that when rapid mercurial action is desired, it is possible to obtain it in this way. But almost invariably, and for every preparation, one-sixteenth of a grain was used, and the observations I have to make refer to this quantity. The injections were made once in 24 hours until the gums were slightly touched, after which, according to circumstances, they were left off altogether or used every second or third day.

I think I may say that one of the most striking results of these experiments was, that we were unable to detect any distinct difference in the action of the various forms of mercury either locally or generally. The observations of others had led us to expect that the bichloride would prove exceedingly irritating and unsatisfactory. We knew that various attempts had been made to overcome this by combining it with morphia, and by converting it into an albuminate. When, therefore, in the first series, we found that the pain and induration which these and all other mercurial preparations caused were very considerable, we did not venture to experiment with the pure bichloride. We had found that at the point of injection there arose a swelling sometimes as large as a walnut, painful, and often persisting for two or three weeks. This was less palpable and less painful if the injection were made deeply, and on the back, shoulder, or buttocks, than if it were simply subcutaneous and on the limbs; but the difference was probably due to the relative exposure to injury and the sensitiveness of the parts. Out of more than two hundred injections there was only one instance in which a tardy suppuration followed the little operation; but the condition of a patient with twelve or fifteen of these tender spots was anything but pleasant, and, in point of fact, a mutiny threatened, and the injections were discontinued. Our second series coincided with the first in so far as similarity between the actions of the various preparations was concerned. But the local irritation was much less. It seemed to be of trifling importance in what portion of the body, or at what depth, or with what preparation, injection was made. The pain and induration were always very small, and the maligned bichloride which we now ventured to try was very little worse than its neighbours. I have found it difficult to determine the reason of this difference in result; but I believe that it was due to the most scrupulous attention to cleanliness on the part of my house-surgeon, Dr Kirk.

In all other respects the results of the observations were parallel. I shall not enter into details of cases, but sum up the advantages and disadvantages which appeared to attend this mode of using mercury.

Half a grain was the smallest amount which sufficed to touch

the gums. A grain and a half was the largest necessary. The average of all the cases gave a little over fourteen injections as the number required to produce salivation. The precision with which mercurial action was produced was thus very satisfactory, and the total amount of the drug introduced into the system was exceedingly small. Gentle salivation could be afterwards kept up with very great certainty by injecting every second day, with slight variations in the sense of greater or less frequency. Only in one instance did the remedy seem to produce disorder of the stomach and bowels, but as is invariably the case with mercury, lassitude and anæmia manifested themselves if the cure proved tedious. On the other hand, I do not think the therapeutic effects differed from those of other modes of exhibition, and there is no doubt that patients strongly object to the continued use of the needle and the production thereby of a number of spots, which, if not exceedingly painful, are yet tender on pressure.

Fumigation.—In directing our attention to mercurial fumigation we did not fail to try various forms of apparatus. We found that for practical purposes the best was a simple porcelain dish divided into two compartments, one containing the mercury, the other an ounce or two of water. This is placed on a tripod with an ordinary spirit lamp below it. Along with the patient it is enveloped in blankets or tarpaulin, being covered by a large cage if the patient be in bed, or by the chair if he be seated. The water is not essential to the process, but seems to render it more comfortable, and aids the production of perspiration. Calomel is in all respects the best preparation of mercury that can be used.

We thought it well, in the first place, to endeavour to estimate the value of fumigation alone as a means of introducing mercury into the system. If twenty grains of calomel be volatilized, and the patient be exposed daily to its influence for twenty minutes, no signs of mercurial action are produced; and even though forty or sixty grains be used, the gums are touched in comparatively a small proportion of cases, and after a long time. The baths were given in the general ward. The patient was absolutely nude, and was put to bed afterwards for an hour or two.

If on the other hand, the bath be administered in the manner recommended by the advocates of this method,—that is, if the patient be directed to take each time a few inhalations of the fumes, if ablution be forbidden and the underclothing impregnated by the fumes be retained in contact with the skin, then with only twenty grains of calomel salivation may be produced in most cases with considerable rapidity.

The evident conclusion from these experiments is, that while

the quantity of mercury absorbed by the skin is insufficient to produce palpable evidence of its presence in the blood, yet so much is thereby taken up as to make it easy to produce this action by other means, such as inhalation. We had incidental proof of the risk of inhaling too freely, and so of the impropriety of attempting to introduce mercury by the lungs alone. One patient who, contrary to orders, kept her head under the blanket for several minutes (she said as long as she could) suffered in consequence from a very severe attack of bronchitis. No doubt the few breaths which are necessary to the method may be drawn with impunity, at least, so far as evident injury is concerned; but it is a subject for consideration whether administration by the mouth might not be advantageously substituted for inhalation. We found that the exhibition by the mouth of a teaspoonful of the liquor hydrargyri bichloridi twice or three times a day produced salivation with great certainty and rapidity when combined with mercurial fumigation, although that quantity is very slow of doing so when given alone.

The therapeutic effects of mercurial fumigation are very good. A certain class of cases improved, I think, more rapidly in this way than under any of the other common modes of treatment. It apparently acts by three distinct processes—the introduction of mercury into the system, the production of diaphoresis, and the topical effect of the remedy. Of patients who come into the Lock Hospital, there can be no doubt that a large number derive benefit simply from the improved dietary, greater cleanliness, and attention to the primæ viæ. In a considerable number of cases I have limited the additional treatment to the daily vapour bath, and the benefit derived from the copious perspiration thus induced has often been very striking. Experience, moreover, would lead me to believe that the action of mercury, however it be administered, is much improved. both in hospital and private practice, by some such means. The Turkish bath, two or three times a week, is admirable, but for the risk of catching cold after it, an objection, however, which cannot be raised to the daily vapour or hot-water bath at home. But the method of fumigation has yet another advantage. In the experiments which we made without inhalation we were struck by the satisfactory progress of our patients. I was not inclined to believe that the action was purely local, because I found that sore throats, iritis, and other manifestations of syphilis which did not come in contact with the vapour, improved under the treatment; but I felt certain that it did produce a topical effect. Of all forms the affections of the skin are those which receive benefit most speedily, and in general eruptions I have frequently noted that those portions which were most exposed to the mer-

curial fumes were precisely those which most speedily disappeared. In one case of squamous syphilide affecting the body and scalp, the disease disappeared on the body before any improvement whatever had shown itself on the head. Such an effect, indeed, one would be prepared to anticipate from the well-known beneficial action of mercurial unguents, and lotions, and powders, in syphilitic diseases of the skin.

But, on the other hand, mercurial fumigation doubtless has its disadvantages. The absence of ablution and the retention in continuous use of the impregnated clothing, if they be insisted on, render it peculiarly distasteful to those who are of cleanly habits. But whether or not these precautions be used, if the hot vapour baths be continued daily for any considerable time, they come to have a seriously-depressing effect on the patient. This languor and anæmia, moreover, to which syphilitic patients in hospital are prone, to which mercury conduces, and which is intensified by the vapour bath, occurs in spite of the most sustaining and tonic measures. I have already pointed out that there is a period in the continuance of mercurial administration, varying in different individuals and in different phases of the disease, at which the remedy not only ceases to do good, but actually becomes injurious, and tends to prolong the syphilitic manifestation. This injurious effect, it seems to me, shows itself soon and distinctly under mercurial fumigation.

Our observations on the method of treatment by mercurial inunction present little worthy of remark. In some patients we used the ordinary mercurial ointment, in a good many the ten-per-cent. oleate of mercury, and I think that the only difference to be noted is that the latter is somewhat less uncleanly than the former. Having prepared the skin by soap and water, and the patient by a gentle aperient if necessary, we proceed with the treatment. We divided the body into ten regions, and used a different region for inunction each day. The rubbing was continued for twenty minutes on each occasion. The ordinary physiological effects were, on the whole, produced with regularity. There were, however, notable exceptions. Thus, in one patient we had distinct salivation on the fifth day, while in another the gums were not touched, after daily rubbings, for a period of four weeks. Doubtless peculiarities of this kind present themselves, whatever be the mode of introducing mercury into the system, but they appear to be more common with inunction than with other methods. The divergence may, perhaps, be explained by the fact that we have here to deal not only with constitutional idiosyncrasies, but also with variations in cutaneous absorption. Doubtless in most cases these peculiarities may be expected to neutralize each

other, but when coincident in one direction or other, they produce the extreme results to which I allude. This method has the disadvantage of being uncleanly, and it is not free from the risk of producing injurious effects on the digestive organs and general system. I do not think that its therapeutic action differs in any material respect from that of other methods.

I shall not detain you with details of cases wherein mercury was administered by the mouth. Every one, doubtless, has his favourite preparation and peculiar mode of using it. I shall venture merely to indicate certain general conclusions.

In the first place, I would observe that the advocacy of particular preparations is not warranted by any special advantages which are possessed by one as compared with another. In suitable dose, all the preparations in common use may be made to touch the gums and produce the desired constitutional and therapeutic effects with nearly equal facility. With all of them there is the same tendency to produce digestive and other derangements, and if proportionate doses be administered with equal frequency, this tendency is not markedly greater with one preparation than with another.

In the next place, there can be little doubt that, for the end in view, small doses, frequently repeated, are better borne and more effectual than larger administrations at longer intervals. With the bichloride, for example, a teaspoonful of the liquor given four times a day is less likely to produce irritation than a dessertspoonful twice a day. Lastly, it is of importance that the patient be prepared for the exhibition of the drug by careful attention to the general state.

There only remains for me to say a word as to the preference to be given to one of these modes of administration over the other. I think there can be no doubt that the majority of cases in private practice, when they require mercury, are best treated by the oral method. If small doses be given in the first place, and gradually increased in frequency till the desired effect be produced, the chances of gastric or intestinal irritation are easily avoided, and salivation can be prevented by the use of chlorate of potash, or alum wash after each dose. The patient is thus permitted to follow his usual avocation, and the remedy may be continued with proper precautions, as long as may be needful. With the other methods it is difficult, from their inconvenience, uncleanliness, or disagreeableness, to induce the patient to persevere sufficiently long; and one strong objection to the routine use either of mercurial fumigation as practised by Mr. Lee, or inunction as practised by Sigmund, lies in the necessity for confinement to the house, and even for a considerable portion of the day to the recumbent position. Exercise in the open air, a nourishing dietary, and even a certain amount

of stimulation, especially in chronic cases, are of great importance, whatever be the way in which the mercury be given. I have already said that free action of the skin is very desirable, and the occasional use of the vapour bath is a good method of securing it. In obstinate cases of skin affection, an occasional mercurial vapour bath may advantageously be added, and in hospital cases mercurial fumigation is probably the most efficient plan, at all events, to begin with.

I do not think that inunction and subcutaneous injection hold out sufficient inducement to employ them, otherwise than as exceptional measures in cases of peculiar weakness or individual idiosyncrasy.—*Edinburgh Medical Journal, Aug.* 1878, *p* 98.

MIDWIFERY,

AND THE DISEASES OF WOMEN AND CHILDREN.

53.—ON THE PROPER MANAGEMENT OF TEDIOUS LABOURS. AND PARTICULARLY ON THE USE OF THE FORCEPS IN THESE.

By Dr. G. HAMILTON, Falkirk.

The experience, capability, and knowledge of the medical attendant must evidently have a great deal to do with the resulting mortality. In the earlier years of my own practice, in the hands of my assistants, and of midwives, the fœtal mortality has been considerable. When, however, the series of 731 successive children born alive, which I have given (British and Foreign Med.-Chir. Review, Oct., 1871), began, I had been upwards of fourteen years in practice, and had used Ziegler's forceps for a long time. I had, therefore, great advantages over a beginner, who probably has neither at first much confidence in himself in applying instruments, nor, what is often of as much importance, has obtained the entire confidence of his patients, especially in districts where instrumental aid has been rarely given. It has, on this account, appeared to me that it conveys an unfair idea of the value of the forceps as a conservative instrument to include the first few years of a practice in statistics which are to furnish reliable information. Much, undoubtedly, must depend on the training, and especially, I think, on the system of using the instrument which has been adopted, for I shall shortly have to show that in two instances the results, even from the first, have been highly satisfactory, at least with the forceps.

Indeed, I lay the greatest weight of all on the instrument used, and the *mode* and *time* of using it, in procuring for us successful results. The instrument I use and recommend must be familiar, from the drawings at any rate, to those who have read my papers referred to, and I have spoken in these so fully of the great advantages it possesses, that I am almost ashamed to say more on the subject. This form of forceps is, however, so intimately connected with what I consider distinctive in my practice, that, without such an instrument it is impossible, if it should be considered desirable, to apply it as I have done, for I find I almost invariably place the blades antero-posteriorly,

or nearly so. The advantages of grasping the head in this way I think, and have attempted to show, are enormous, and I feel confident, from an experience of half a lifetime, that the practitioner who once gets and uses the forceps as I have directed will never feel that he needs another. Practitioners who read this may be inclined to say to me, "But I cannot use my forceps as you do, for they are double-curved, and were recommended to me by my teacher as the best made." Very true, possibly; but my reply would be, "Try to remedy this state of things, and get from Mr. Young, of Edinburgh, who made mine, a forceps such as I have described, and I think you will find it the best investment in instruments you have ever made. Get, I would say, the three blades, for, though I have generally managed to dispense with the short blade, it gives a good hold where the os sacrum is very prominent, and it can be laid aside when the labour advances."

The stronger instrument I have latterly used possesses decided advantages over that originally designed by Dr. Ziegler, from its greater length and strength, and from other circumstances which I have mentioned. Since my last paper appeared I have met with a case which showed this very decidedly. I was called to a patient in labour a short distance in the country, and put my old forceps, because it was not so heavy as the other, in my pocket. This case proved very tedious, and I had to use the forceps. This I did assiduously for some time, but found that I constantly missed my hold. I, in consequence, despatched a quick messenger for my stronger instrument, and with it at once finished the labour.

I still notice in the journals the complaint that "the forceps slipped, and perforating or crushing had to be resorted to;" but I almost never have to complain of this with my instrument. The hold which I get is generally perfect. The solid blade, as I have mentioned before, also seems to me to be an improvement, for there is no catching in introducing it or in pulling with it, and there is not even a scratch left where it has been fixed. Two of my recent cases had the face to the pubes, and the solid blade was at first in both applied over the face, without the slightest injury to the skin.

What has been said will show the kind of instrument I use and recommend, and its *mode* of application; but a distinctive feature of my practice is also—

Fourthly, the time in the course of the labour, when the instrument is resorted to.

This I find is, in the great proportion of cases, when an ear is near the pubes, before the face has taken the turn into the hollow of the sacrum, if it ever does that. The last time, many years since, I had to use the forceps when the face was in this

latter position was when called to assist a midwife. In my own practice I usually anticipate this stage, so that formerly mine would have been classed as long-forceps cases. Usually, I find that the forceps is introduced, measuring from the external parts of the mother, about nine inches, while the old short forceps measures only ten altogether. I always, when I get my hold, which is generally effected in a minute or two, or less, use traction at first, and am pleased if the face takes the turn into the hollow of the sacrum. If it does not, I use leverage, and make the head revolve round the pubes until it is delivered.

Having given these details, which are really in great part repetitions of what I have said before, I will now state the results of my practice since September, 1871 (the last date to which the two series were brought up, by this general statement, that there had been no fœtal death in forceps cases, occurring in the usual ratio, for ten years), and which I would much rather, with a few explanatory observations, have given in the form of notes to a reprint of Dr. Dunster's paper. I thought, indeed, that I had written enough on the subject, and that those who were not convinced by the statements I had made of the value of the forceps as a conservative instrument were a *little* impervious to reason. This series, extending to 1878, although now necessarily small from my advancing age requiring that I should gradually withdraw as much as I could from this line of practice, is yet, I think, the most valuable in some respects of any I have given to the profession; for I have had entire confidence in my mode of treatment and in my instrument, and my patients, I think, have had as entire confidence in me. I think I can especially recommend it to the younger members of the profession, as giving a rule in regard to the ratio in which they must make up their minds to have to use the forceps in a mixed healthy population, such as I practise among, in order that a very low rate of mortality may be attained; in other words, that a satisfactory practice may be followed.

The series (not an isolated one, it will be remembered, but the third successive one of the same kind, extending over thirty years) then, included one hundred and sixty-seven consecutive cases. All the mothers made excellent recoveries, and all the children were born alive, except one, in a forceps case. The forceps was used in thirty-one cases, or one in between five and six. Twelve of the children were born just before I arrived, but no accident happened to either mothers or children.

In making a few remarks upon this series, I shall speak first of the mothers.

All the mothers here recovered excellently well; but I have to repeat, what I have so often and strongly insisted on before,

that this is really at best a very fallacious test of the value of a practice, and that the question as to the cause of mortality here has to be settled rather by opinion than by statistics. In proof of this I may say that, during the time this series has lasted, I had to see in the practice of others five cases in which the mothers died. Three of these occurred nearly at the same time in the practice of a very cautious midwife, who has assured me that the labours were all of the easiest possible description. The two others were in the practices of professional brethren, and the labour in one of them, I was also assured, was natural and easy. On the fifth I shall say a few words. I was called to a woman in labour, three miles distant from any medical man, and found that she had been in labour from the previous day, and was in such an exhausted state that her friends had become seriously alarmed about her. I was shortly afterwards joined by the gentleman in attendance, and at once said that I thought he should lose no time in applying the forceps. This he attempted to do, using the double-curved instrument; but in a short time gave up the attempt, as he was unable to fix the blades. I had then to try to deliver the woman, and did so, with my own forceps, in a few minutes, the child being putrid. Now here, if I were asked my opinion as to the cause of the patient's death, I would say, without the slightest hesitation, that as far as the forceps was concerned, it had nothing whatever to do with it; that, in fact, the use of this instrument gave the woman by far the readiest and best means of getting out of her perilous position. If I were asked, then, to say what I thought had been the cause of death, I should say again unhesitatingly that there might have been other factors in operation, but that the chief one undoubtedly was the long and exhausting effects of the labour before the forceps was used. To be plain, indeed, to talk of the use of the forceps, as I now use it, being in such cases as this a probable cause of death, seems to me simply nonsense; not that I have not seen in my time some deplorable cases of this kind, but these were abuses of the instrument, or arose from defective knowledge and instrument, and do not belong to the use of the instrument as we are entitled to speak of it now. It must be admitted, on the other hand, from the statements of many writers, that where baneful epidemic influences have been absent, protracted labours and slight instrumental interference are compatible with a low maternal mortality.

I think I am entitled to say that, when required, the forceps can be used in every sixth or eighth case with perfect safety both to mother and child; nor do I think (though Dr. Playfair has said that "nothing is more remarkable in modern midwifery than the results published by Dr. Hamilton, of Falkirk, which

have not attracted nearly sufficient attention") that there is the least difficulty in any *capable* practitioner attaining similar results under like circumstances, if only he will follow the rules that have guided me, and use the same instrument as I have done. I have no secrets to conceal, and I think no more than average dexterity in manipulation is required for the purpose.

In concluding what I have to say as to my own practice, therefore, it may be useful to summarise, to the practitioner or beginner, what I consider the principal rules that have to be attended to in the management of tedious labours:—

1st. Remember that it is the length of the labour that especially proves hurtful to mother and child. This holds good as to both, but especially as to the latter. The first stage of labour should be little, if at all, interfered with; but its length should be a guide as to the second, which should not usually be allowed to be prolonged much beyond two hours, and much less when the first stage has been long and exhausting.

2nd. Usually the shortening of the second stage has been effected by supporting and pushing up the uterus over the head, or by the use of the forceps.

3rd. Get rid of the double-curved forceps. This I consider to be vital; I have used Ziegler's straight forceps (now slightly altered) for some forty years, and I find that the pattern I now use supplies every want I have felt, in my own practice, and in assisting others, since 1832. (See drawings in British and Foreign Med.-Chir. Review, Jan., 1872.)

4th. If the rule as to shortening the second stage of labour be attended to, and the forceps is required, an ear will *generally* be found at or near the pubes, and, when an ear can easily be felt, I usually consider the case safe. Here slip in the blade No. 1 (without fenestra), and the other will usually without difficulty slide into its place.

5th. It will thus be seen that I generally place the blades antero-posteriorly, or very nearly as represented in Smellie's 4th Plate, though the instrument used is different.

6th. If the head should be higher, the forceps can still be used excellently well antero-posteriorly; but get hold, in this case, wherever you can, and by traction advance it into the pelvis. Then shift the blades and place them over the ears, or over the ear and parietal region, still using traction. If this should not succeed, try at the same time to turn the face (to the right or left as the case may be) into the hollow of the sacrum. If this again should fail, try traction and leverage combined (see *loc. cit.* page 176), by making the head, without turning, revolve round the pubes. When this last force can be brought into play it is the most powerful of all in effecting delivery. When the face is towards the left, of course, from the presence

of the rectum, it is not so easy to make the head turn with the face into the hollow of the sacrum as when the face is towards the right. In such a case I have occasionally pushed the head up and turned the face from the left to the right side and then delivered. When the face is towards the pubes the same can be done.

7th. Podalic version, perforation, and crushing, remain as our other resources; but as to these I have nothing to say, except that where the forceps is properly used they will very seldom be required.

Even in breech and footling cases, when the head comes into the pelvis, Professor Busch, of Berlin, has shown that the forceps can be used very effectually.

8th. Refuse to give chloroform or ergot in the first stage, and in the second as seldom as possible, the first near the close of labour, the second to increase the pains and bring the head within reach of the forceps, or at the close to anticipate flooding.

9th. Though it may be necessary occasionally to introduce the forceps *within* the uterus, try to avoid this as much as possible by assiduously pushing up the uterus with the fingers or hand both anteriorly and posteriorly, for the application of the instrument is thus made easier, and the risk to the mother is lessened.

10th. If the child should be seriously asphyxiated when born lose no time with other measures, but instantly inflate its lungs with the mouth. (Vide Edinburgh Med. Jour., May, 1855.)

11th. Remember when the face is to the pubes, that rotation to the left is sometimes easier than to the right, the reason probably being that the occiput encounters the rectum when the face is turned to the right. In 1841 a case of this kind foiled me, and I had to perforate. My 732nd case, also, had the face to the pubes.

12th. If flooding follow delivery, and supra-pubic pressure and emptying the uterus of clots fail, use pressure and *counter-pressure*, by introducing the right hand under the uterus, and placing the left above the pubes; and keep this up till the hemorrhage stops, sometimes little short of an hour. (Vide Edinburgh Med. Jour., Oct., 1850.)—*Obstetrical Journal, June* 1878, *p.* 141.

54.—ON DIGITAL DILATATION OF THE OS IN LABOUR.

By Dr. WILLIAM STEPHENSON, Regius Professor of Midwifery in the University, Aberdeen.

The question which I propose to consider is, how far, and in what manner, may we with advantage aid the dilatation of the os by means of the examining fingers?

It will be observed the question has no reference to the *accouchement forcé*, where we desire to take the progress of the labour into our own hands, or where from pathological conditions we are forced to interfere. It is not interference that is contemplated, but only aid, in cases where, left alone, Nature would ultimately effect delivery, but with a greater expenditure of strength than is necessary or, it may be, safe. In a large proportion of cases no such aid is required, but many constantly occur, where it is evident that there is an expenditure of effort out of proportion to the work done, and where, by timely and properly directed aid, the patient can be saved much unnecessary, if not injurious, suffering.

There are, I believe, few practitioners of experience who do not employ, at times, some form of digital dilatation in the first stage of labour. In the manipulation necessary to complete the diagnosis, it is a common experience that the os is thereby often sensibly enlarged, and the experience so gained leads one to employ more direct effort towards dilatation, when the natural process proves tardy. The method of applying the aid varies, but there are few who have not adopted some mode, and who have not found that a gentle dilating force may in many cases be employed with advantage.

If we are to judge however by our text-books, digital aid to dilatation has failed to gain any position in the obstetric art. In some of the older writers, in the beginning of the century, the subject is discussed; but in recent standard works it may be said to have been entirely dropped, while yet they faithfully retain, in almost stereotyped form, bloodletting, baths, belladonna, and like remedies, which no one now ever thinks of using.

Dr. Playfair alone, in his recent work refers to the question, and his remarks I would take as an introduction to the subject. He says:

"Artificial dilatation of the cervix by the finger has often been recommended, and has been the subject of much discussion, especially in the Edinburgh school, where it was formerly commonly employed. It is capable of being very useful, but it may also do much injury when roughly and injuriously used. The class of cases in which it is most serviceable are those in which the liquor amnii has been long evacuated, and in which the head, covered by the tightly stretched cervix, has descended low in the pelvic cavity. Under these circumstances, if the finger be passed gently within the os during a pain, and its margin pressed upwards and over the head, as it were, while the contraction lasts, the progress of the case may be materially facilitated. This manœuvre . . . . if properly performed, I believe to be quite safe, and often of great value."

It is certainly strange that the writer, who has thus, in a short compass, expressed the same views as Professor James Hamilton, of Edinburgh, and has found the treatment to be "quite safe and often of great value," should in a previous part of his work, when referring to the recommendations of Hamilton, have stated that they were "*properly* objected to by the great majority of obstetricians."

What Dr. Hamilton recommended was not forcible dilatation by means of the introduction of the fingers, but only aid by means of "pressure on the resisting band of the uterus with the point of the finger during each successive pain." Also where the undilated uterus is being forced down into the pelvis during every pain, he says—"It is necessary by the application of two fingers to the edges of the os uteri, to retain the uterus, *in situ*, during every pain, till the head of the infant pass into the vagina. The practitioner is to confine his assistance to the attainment of this object alone, and he is most particularly *to guard against any attempt at forcing open the os uteri.*"

Professor Burns, of Glasgow (1824), in his eminently practical "Midwifery," which even yet well repays perusal, advocates the same views, but goes somewhat further, recommending when the cervix is "thin, lax, and soft" to aid dilatation by the introduction of the fingers within the os. The objections which have been brought against the recommendations of these writers are based on a misunderstanding or ignorance of what is really taught by them.

A sound principle of treatment is laid down by Dr. Burns in the following words:—"Unproductive action ought never to be allowed to continue so long as materially to impair the action of the womb. If we cannot safely render the action more efficient, we must endeavour to suspend it; by which the womb recruits, and the retarding cause may in the meantime be removed or cease to exist."

The selection of the line of treatment in protracted first stage, whether to aid or suspend action, is made to depend on the condition of the cervix and the character of the pains. If the cervix is not in a condition where ready dilatation can be expected, or the pains are irregular and unsatisfactory, delay is recommended. Dr. Hamilton in particular dwells on the evils which are liable to occur from long duration of ineffectual pains in the early stage of labour. A clear conviction however, that labour has been in actual progress for some time, and a continuance of regular uterine contractions, are insisted upon by him as essential before any digital aid is warranted. Where the labour is doubtful or the pains irregular and feeble, opiate enemata are recommended.

In the next place, before digital aid is to be used, they clearly

recognise that certain changes in the cervix must have taken place. It must be certain that the uterine action has begun to act *directly* on the parturient ring, and that its tissue be "thin and soft." Digital aid is therefore not to be employed, while yet the cervical canal is not wholly obliterated; or whilst the os is small and the lower uterine segment is not sufficiently expanded to permit of the presenting part coming in contact and engaging with the cervical tissue. Whilst these conditions exist, and interference is called for, bloodletting was the resource which naturally was most relied upon at the time they wrote. When this was inadmissible an opiate is to be given. They distinctly caution against anything like digital dilatation under such circumstances. Nowadays, we may read chloroform or chloral for blood letting, and the majority of practitioners will concur in their views.

When the cervical tissue has become thinned and the os remains small, very gentle dilating action with the fingers will frequently succeed in making a considerable increase in the size of the os. This, when judiciously done, is not productive of any mischief. Roughly used it may be otherwise, but we speak only of its judicious use. It is sometimes necessary for the purpose of fuller diagnosis, and uterine action may thus be stimulated. Dr. Burns recommends it, but with due caution. This, however, I may remark, is not truly digital aid, for labour is not directly facilitated thereby, except it stimulates contraction, the tissue which has thus yielded to the pressure of the fingers, would as readily yield to the pressure from above, did there not exist some other obstacle to progress. The cause of the delay is not the thin tissue which yields so readily to the finger, but the want of dilatation of the internal os; or it may be that the delay is due to the membranes being still entire; abnormal direction of the axis of the uterus has also the effect of retarding the dilatation.

The condition under which digital aid can be most effectively and properly employed is when, after rupture of the membranes, the cervical tissue is stretched over the head, and it is specially necessitated when the cervix is being with each pain pushed down before the head instead of opening up and receding over it. Although differently expressed, this is what was more particularly referred to by Hamilton and Burns. "It is done best," says the latter, "by pressing on the anterior edge of the os uteri during a pain with two fingers, with such moderate force as shall not give additional pain, and shall appear to excite the natural dilatation as much as to produce mechanical opening. By doing this for several pains in succession, or occasionally during a pain at intervals, according to the effect produced and the disposition to yield, we shall soon have the os uteri completely dilated."

Such are the teachings of Professors Hamilton and Burns regarding digital dilatation of the os uteri. In justice to them I have given their opinions at some length, that it may be seen that these have not "*properly* been objected to by the great majority of obstetricians," that they inculcated the greatest caution and warn repeatedly against using force, and that they go no further than Dr. Playfair does himself in recommending a manœuvre which he properly believes "to be quite safe, and often of great value if properly performed."

There is, however, a deficiency in the recommendations as laid down by the above authors. The manœuvre may at one time be tried and found of great value, whilst at another it either fails simply or does harm. This is sufficient of itself to explain why it has been discredited by many who have tried it. The treatment as described takes no cognisance of the position of the fœtal head, and being applied irrespective of the varying conditions in the normal mechanism of labour, it lacks the precision necessary for success. We aid when we can facilitate the normal movements of the head, we but hinder or obstruct if we derange the mechanism.

When in normal labour the membranes are ruptured, whilst the os is not obliterated, the posterior part of the head clears the os first, the anterior being still held back by the rest of the cervical tissue. There is a clear gain by this movement, the head is more flexed, a smaller diameter is presented, and the rotation forward of the occiput becomes easy. This is the movement we must not disturb, but if possible facilitate. In aiding labour, therefore, at this stage the support and upward pressure must be exerted only so as to push, as it were, the lip of the cervix *over the occiput*; it must never be done over the forehead. A careful diagnosis of the position of the head must be made, and the direction of the support determined accordingly. The part selected should never be the *anterior* lip, as described by our authors. In the first position of the head the part corresponds with that opposite the left thyroid foramen, and comes readily to the fingers. In the second position it is opposite the right thyroid foramen. In the occipito-posterior position the treatment is carried out less readily, but can still be accomplished, the direction of the force being towards the corresponding iliosacral synchondrosis. The success of the manœuvre is dependent upon aiding the occiput to descend first. If then it be practised at haphazard, and always in the same direction, failure is certain to follow in many cases. By its improper use the anterior portion of the head may be enabled to lead, and the normal mechanism is disturbed. If the pressure be exerted at the side of the head, as it will be if directed immediately behind the pubes, dilatation does not

take place, and the head is really held back. The pressure must be exerted only during a pain, and the patient be directed to bear well down. The efficiency of the uterine action is thereby greatly increased.

This method of aiding labour need not be confined to protracted cases; gentle and properly directed support is of advantage in all. The force employed need not be more than is represented by the word support. A due amount of chin-flexion is secured, and upon this depends the facility of the subsequent rotation of the occiput. More especially is this of importance in the occipito-posterior positions; if attention be paid to secure early and full flexion of the chin no difficulty will be experienced in the rotation. Before it is possible in the latter cases to slip the cervix over the occiput an initial degree of chin-flexion is necessary. If the fore part of the head is on a lower level, the occiput is beyond the range of the fingers, and the manœuvre is impossible. Aid may then be extended by pushing the forehead upwards during the interval of the pains and retaining it as far as possible in that position by pressure during the contraction of the uterine walls, and repeating this manœuvre until the posterior fontanelle can be felt.

Greater precision may be given to our opinions regarding the use of digital dilatation by the more recent advances of our knowledge regarding the changes which occur in the body and cervix of the uterus during the first stage of labour through the researches of Litzmann, Bandl, Braune, and Luschka. The lower uterine segment of the body of the uterus and the tissues of the cervix both undergo dilatation; but in the former the transverse expansion is associated with marked shrinking or shortening of its longitudinal diameter, whilst the latter is greatly stretched and lengthened. The division between the uterine segment and the cervix is what is known as the internal os, whilst the external os is what we recognise as the os of ordinary obstetric language. The distance between these, as shown by Braune's frozen section, may be as much as four inches. In normal labour the dilatation of the internal and external os go on simultaneously, the former slightly in advance of the latter. This relation however may be deranged. The internal os may be fully dilated whilst the external is very small. We cannot however get any degree of dilatation of the external os without the previous opening of the internal. The opinion which I would advance is this: that digital dilatation can exert a beneficial action only upon the cervical tissue, we cannot by this means aid the expansion of the lower segment of the uterine walls. So long, therefore, as delay is associated with incomplete dilatation of the internal os digital interference should not be employed; but when delay is due to want of

dilatation of the external os whilst the expansion and retraction of the internal has well advanced, we may expect benefit from artificial means. The degree of dilatation of the internal os I believe we can estimate by the condition of the upper portion of the vagina. When the former is complete the latter also is fully expanded and drawn upwards. If the external os has not been simultaneously dilated, the cervical tissue will be felt stretching across like a diaphragm, with a varying degree of thickness and resistance. If however the internal os be not fully dilated the upper portion of the vagina will be found lax and attached near to the os, or curving in towards it. Digital dilatation will then have no beneficial effect unless it be by stimulating the uterine contraction. But when the diaphragm is developed it will yield to judicious gentle manipulation; if the os be small by a rotatory action of the fingers; when once half-way dilated, and the head in actual contact, by support and gentle pressure of the lip in the direction of the occiput. A clear conviction should also be established that the cause of delay is in the cervical tissue only, and not due to want of rupture of the membranes, or to malposition of the head, to abnormal direction of the uterine axis, or to narrowing of the pelvic brim. Many cases of tardy dilatation are due to these causes, and of course cannot be aided by artificial dilatation.

By care in diagnosis the time when digital dilatation may be employed with advantage can be readily determined, and if practised as I have indicated, with due regard to the mechanism of labour, it may be employed with precision and safety. It affords material aid, increases the effective character of the pains, insures and facilitates the normal movements of the head, and if properly employed is free from all danger to the patient. It is a proceeding, therefore, which merits recognition at the hands of obstetricians more than it has hitherto received. By extending our aid in the first stage of labour by watching and furthering the normal mechanism, I am confident that we may very materially lessen the frequency with which in recent times instrumental interference is deemed necessary.—*Obstetrical Journal, August* 1878, *p.* 273.

55.—ON POST-PARTUM HEMORRHAGE,

WITH NOTES OF THREE CASES SUCCESSFULLY TREATED BY COMPRESSION OF THE ABDOMINAL AORTA.

By Dr. M. M. BRADLEY, Jarrow-on-Tyne.

Hemorrhage will occur; and although we cannot at all times prevent such a melancholy catastrophe, we can be ever on our guard against it, so as to reduce its occurrence to a minimum, and by anticipation avail ourselves of those remedies and means

which experience has shown and practice proved to be the most beneficial for that purpose. More than a quarter of a century ago, Dr. Ewing Whittle pointed out to the members of the Medical Society of Liverpool, "that the probable occurrence of post-partum hemorrhage in any case of parturition could be diagnosed beforehand, and, being diagnosed, could be prevented." This assumption he made upon the character and duration of the pains, which are stated to be "sharp, quick, and cease almost suddenly, having an interval between the pains, long in proportion to the length of the pain." This I believe to be in perfect accord with the physiological action of muscular tissue; and thus we have in the delicately constituted woman with weak and flabby muscles, or in the robust and powerful woman, whose muscular irritability is exhausted by too long or too powerful action (or both), short and quick contractions with a long interval occurring between each contraction, so as to enable nature to restore the force expended. It is against these two conditions that we are to be constantly on our guard.

The former must be met by the judicious and timely administration of ergot of rye—always provided no obstacle intervenes—and the latter by the skilled and timely delivery of the patient, either by manual or instrumental interference. Far be it from me to advocate the administration of ergot or the use of the forceps on any other ground than that beneficial to the patient, because I thoroughly believe in the axiom taught by Blundell that "meddlesome midwifery is bad," and I am equally aware of the injurious effects to mother and child which may arise from the injudicious employment of either; still, I am convinced that the withholding of either under prejudice or cowardice is a very fruitful source for many of the evils that happen in child-bearing. If we would act according to the law which governs equable, permanent, and complete muscular contraction, by removing from within and around the uterus all foreign matter, taking care to follow down the uterus during the expulsion of the fœtus, as taught and practised by Clarke, Collins, Beatty, and others; and not to empty the uterus too rapidly of its contents, but leave it to expel the body and feet of the child as insisted upon by Barnes, we should then obtain what might be justly termed a complete state of prophylaxis, and certainly hemorrhage post-partum would be reduced to a minimum.

Supposing prophylactic measures failed, and that the patient is flooding, our first duty is to ascertain the cause, and endeavour to remove it.

If the placenta has not yet been expelled, then the hemorrhage has one of two sources either from some rent or tear in

the paturient canal, or from the placental site, owing to its partial detachment. If the latter, we must aid the uterus in endeavouring to expel the placenta by gentle compression and friction over the fundus; but if we cannot succeed by this method (Dublin method), then no time must be lost in extracting it *secundum artem*. If the bleeding takes its origin from the second source, we must use styptics and plug, the only occasion, in my opinion, where the tampon is justifiable in post-partum hemorrhage; or it may be that the laceration involves the cruræ of the clitoris, and acupressuse or sutures will have to be employed. For cases of this nature, see a very interesting paper in the Dublin Medical Journal for November, 1875, by Dr. Macan.

When the hemorrhage is owing to the presence of a tumour or morbid adhesions existing between the serous and muscular coats of the uterus, as pointed out by Graily Hewitt, the bleeding surface should be swabbed over with the perchloride of iron, after which antiseptic precautions should not be omitted, lest septicæmia follow, although Barnes speaks in favour of the perchloride itself being an antiseptic. But not unfrequently the fault lies in the uterus itself, owing to inertia from some of the causes already mentioned. In order to relieve this condition various means have been advocated, all of which have been successful, and all of which have failed, the chief aim and object of all being closure of the mouths of the bleeding vessels, either chemically or mechanically. Could we but get the uterus firmly, equably, and permanently contracted, hemorrhage could not occur, and the converse of this is equally true. In making this assertion I am aware it is in direct antagonism to the opinions expressed by Simpson and Gooch, the latter of whom says:—"The observing practitioner must have been struck by the little proportion that existed between the want of contraction and the degree of hemorrhage; having found the uterus bulky without any hemorrhage, and a profuse hemorrhage without greater bulk of uterus. Nay, further, I have witnessed a profuse hemorrhage, though the uterus had contracted in the degree which commonly indicates security; and I have ventured to do what is seldom justifiable—separate the placenta before the uterus had contracted, without more hemorrhage than after a common labour."

I cannot help thinking that in making this observation Gooch was mistaken, and that the contraction which is here spoken of, and which is so likely to mislead, was that most dangerous—because misleading—of all contractions, where the muscular fibres act in a state of irregularity and want of equability in the fundus or body of the uterus. This supposition is all the more certain, inasmuch as we not infrequently

meet with so-called "hour-glass contraction—which is just an irregularity of muscular contractility in different planes—in cases of hemorrhage. This condition is well exemplified in my second case, and it is borne out by Burns, who says:—"I have rarely introduced my hand into the uterus without meeting with it (hour-glass, or irregular contraction), whether the placenta had or had not been expelled." Simpson corroborates Gooch in every particular, and we find him adducing the evidence of complete separation of the placenta, when prævia, without any hemorrhage, and also in cases of twins, where one placenta is expelled after the birth of the first child and no hemorrhage follows, although the uterus is not firmly or equably contracted, owing to the presence of the other child. Then if hemorrhage occurs when the uterus is apparently well contracted, and no hemorrhage occurs after delivery, though the uterus is not contracted to its usual degree, what, I would ask, with Simpson and Gooch, "is that circumstance that has so great an influence that its presence can cause a moderately contracted uterus to bleed profusely, and its absence can cause an uncontracted uterus to bleed scarcely at all?" In other words, what are Nature's hæmostatics apart from uterine contractions?

The placenta is expelled by a process of avulsion, and the torn and injured utero-placental vessels act towards the blood as foreign matter causing its coagulation. When an artery is torn it retracts within its sheath, owing to the contraction of its longitudinal fibres; and, the circular fibres contracting, diminish the calibre of the vessel. This diminution is increased by the mechanical action of the blood extravasated in the felty tissue of the sheath. When blood flows for a length of time, fainting follows; as a natural consequence the heart is unable to propel the blood with sufficient force to expel the clots already forming in the mouths of the injured vessels. The permanent closure is effected by the organisation of the "lymph clot" formed at the mouth of the vessel, and between the internal and external blood-clot. This I hold to be the principle upon which compression of the aorta is based, for by compression we either prevent a current of blood through the serpentine vessels, thus allowing the formation of a clot, or the current is so feeble as to be unable wholly to displace it. In either case a clot is formed, and if of sufficient consistency to withstand the reaction, the mouth of the once bleeding vessel is literally hermetically sealed.

But Nature has adopted another and, if possible, more perfect and beautiful means for the prevention of hemorrhage from the veins and sinuses of the uterus—which is a hemorrhage by retrogression—by the position and mechanism of the uterine

veins, although devoid of valves. This natural mechanism was beautifully shown in a case of dissection of a gravid uterus about the sixth month, which came under the observation of Mr. Owen, who states that he "observed that where the veins of different planes communicated with each other in the substance of the walls of the uterus, the central portion of the parietes of the superficial vein invariably projected, in a semi-lunar form, into the deeper seated one, and where (as was frequently the case, and especially at the point of termination on the inner surface) two, or even three, of these wide venous channels communicated with a deeper sinus at the same point, the semi-lunar edges decussated each other, so as to allow only a very small part of the deep-seated vein to be seen. It need scarcely be observed how admirably this structure is adapted to insure the arrest of the current of blood through these passages upon the contraction of the muscular fibres with which they are everywhere surrounded." Goodsir corroborates this statement, and Simpson says that the microscope shows the common contractile tissue of the uterus in the folds of these falciform projections. Let us now turn our attention to the medicinal agents used in the treatment of post-partum hemorrhage; and foremost among these I would mention ergot of rye, either administered by the mouth in the many well-known fluid forms, or hypodermically. I believe almost all systematic writers give ergot the first place, and McClintock says, "Of all the resources however, against post-partum flooding, I believe the most effectual to be ergot of rye." Turpentine is deservedly spoken highly of, and Mr. Bradley describes its effects as "magical." Borax, cannabis indicus, and opium, have also been warmly advocated. Murphy relates the case of a poor emaciated woman who would have died had it not been for the most energetic measures, and he adds, "I think opium in large doses saved her."

Agents which act by Reflex Action.—Foremost among these are cold, either applied externally or internally in the form of ice, as first recommended by Levret, heat, tampons, and the introduction of the hand into the uterus. The latter, when performed with care and due regard to the axis of the outlet, cavity, and brim of the pelvis, is a mode of treatment well worthy of confidence, as by its presence not only is uterine action excited, foreign bodies therein detected, but if the case is so desperate as to require the injection of perchloride of iron, it is essential for the success of that remedy that the tube be directed on the hand up to the fundus of the uterus. Gooch advocated the compression of the uterus between the hands, one being placed inside and the other outside the uterus; but of course, such a procedure could only compress the placental site,

provided the placenta was attached to the anterior wall. Hamilton advocated the compression of the uterus between the hands, one being placed behind and the other anterior to it. This, under the relaxed condition of the abdominal muscles, is attended with no difficulty, and is likely to be productive of good, if had recourse to early, but if delayed, so as to allow the formation of coagula in the mouths of the vessels, then compression breaks up and disengages them, and thus brings about the very evil we wish to avoid. There is still another advantage to be derived from Nature's tourniquet, and that is its employment in compressing the abdominal aorta. As to whether Ploucquet or Saxtorph were the first to compress the abdominal aorta, Barnes and Churchill disagree; but that they, Chailly, Caseaux, and others did it, and relied mainly upon it, there can be no doubt. I now place before you the notes of three cases in which I practised this method with success, and for it I claim the following advantages:—

1st. Nature's tourniquet is always ready, and can be immediately applied. If, as Barnes says, the loss of life is in the loss of the last three or four ounces of blood, then if by this ready method we prevent the loss of three or four ounces, we save the life.

2ndly. It in no wise interferes with or prevents the use or application of any other remedy.

3rdly. It is of all other means the one more directly under our control, and as it is practised when the patient lies on her back (the best position in post-partum hemorrhage) we can observe the countenance, and also see that remedies of a stimulating, nutritious, and medicinal character are diligently and properly administered.

Cruveilhier, Ferguson, and others, compared the uterus after delivery to a recently amputated stump, and certainly they have both this in common—viz., they both present raw, muscular, and bleeding surfaces; and although I do not quite agree with the comparison, still the analogy is sufficiently accurate to enable me to show that if compression of the main artery to the limb is the legitimate and proper means by which hemorrhage is temporarily arrested, compression of the main artery to the uterus must, cæteris paribus, be the legitimate and proper means for temporarily arresting uterine hemorrhage. Again we permanently arrest hemorrhage from an artery by ligature, and the muscular fibres of the uterus through and among which the serpentine arteries run may not inappropriately (from their function at least) be compared to the silk, catgut, or tendon ligature upon an artery, so that if we insure their permanent and complete contraction (which we can do by compressing the aorta until other agents bring about that result) we prevent

hemorrhage, as certainly as we do from the artery of the stump upon which we securely placed the ligature.

From all this it will be readily conceded that in compression of the abdominal aorta, we have a ready and efficient means of arresting post-partum hemorrhage.

Barnes says, "I have occasionally derived advantage from it and look upon it as a momentary resource."

Before leaving the subject of treatment I should add that of the three remaining remedies—viz., galvanism, hypodermic injection of ether, and transfusion, I employed the two former in the only case of fatal hemorrhage which has occurred in my practice during a period of ten years, and out of a number of deliveries exceeding 2,000. Of transfusion I have no experience. Secondary hemorrhage will probably form another communication.—*Obstetrical Journal, Aug.* 1878, *p.* 288.

56.—THE TREATMENT OF POST-PARTUM HEMORRHAGE BY HOT WATER.

By Dr. WILLIAM S. WHITWELL, San Francisco.

[The following is a letter from Dr. Whitwell to Dr. Atthill of Dublin. Dr. Atthill states that he considers hot water injected into the vagina at a temperature of 110° to be a most efficient method of checking post-partum hemorrhage.]

While in the Woman's Hospital of the State of New York during the winter of 1874-75, in the position of house-physician, I saw in the service of Dr. Emmet the hot water vaginal injections used to great advantage in all pelvic inflammations. It was also customary to order these injections for patients who were to undergo any operation about the vagina, that the tissues might become blanched and contracted, and thereby firmer. But on one occasion I had the good fortune to see the almost instantaneous effect of hot water in controlling hemorrhage in a case calculated to try its powers to their utmost. The patient, a weak and very anæmic woman, had a tumour at the fundus of the uterus, which caused almost continual loss of blood. This, which afterwards proved to be a sarcoma, was partially removed by the scissors, the operation being then suspended on account of severe hemorrhage. The patient was immediately turned on her back, and water at about 110° was injected to the fundus by means of a Davidson's syringe. After the first few syringefuls, the water came away clear, and there was no subsequent loss of blood. One drachm of Churchill's tincture of iodine was then thrown to the fundus, and the vagina carefully tamponed with cotton, which when removed showed merely a staining with iodine. I believe that Dr. Emmet's idea in using the hot water was to cause contraction of the womb,

and so partially control the hemorrhage, but that his main reliance was placed upon the iodine.

A short time before this I had heard Dr. Trask read an able paper on the dangers of perchloride of iron, and the use of iodine as a substitute, in cases of post-partum hemorrhage. The case above cited and this paper fresh in my mind suggested to me the use of hot water in similar cases.

I had no knowledge of hot water having been used by anyone up to this time in cases of post-partum hemorrhage; cold, on the contrary, being advised by all teachers and textbooks on the subject.

My first opportunity to test its efficacy was at Breslau in August, 1875, when, through the kindness of Dr. Landau, the assistant at Professor Spiegelberg's clinic, I was allowed to accompany him to a case to which he had been called. He found that the hemorrhage was internal, and that the fundus was high above the umbilicus. The uterus was cleared of clots, and hot water injected as best we could with the imperfect means, an immense German syringe, that we had at hand. However, contraction took place so rapidly after the first injection that the midwife spoke of it, although she did not know why the *hot* water was being used. The womb remained firm and contracted. Two other cases proved to Dr. Landau its applicability, and at the annual meeting at Gratz in September he spoke enthusiastically of this line of treatment. He was told that it had been discarded long ago.

My next opportunity occurred in the service of Professor Brisky, in the Lying-in Hospital at Prag, where my statements were received with considerable incredulity by the assistant, and it was with some difficulty that I could induce him to use the water hot enough, he being fearful of burning the patient. The first case was a success, and since that time it has been thoroughly tried and accepted as the best treatment, and a large number of cases have been reported. Favourable reports also come from Berlin, and trials are being made in Strasburg, so that I was a little surprised to see that the use of that dangerous agent perchloride of iron is still advocated, and that hot water is not even spoken of.

In only one case have I found the uterus failing to contract almost instantaneously. After performing craniotomy, wishing to wash out thoroughly, and also to cause rapid and fair contraction, I passed my hand with the syringe into the uterus. I was a little startled, after injecting a moderate amount of water, to find that I could not touch the sides of the uterus. Fearing that I had failed, I was withdrawing my hand, when the water gushed out, and the womb had contracted firmly. It was then apparent that my wrist had prevented the return

of the water by obstructing the cervix, and that the uterus had in consequence become dilated.

Would it be possible in a case of transverse presentation, the liquor amnii having come away, to render the turning easy by the replacing of the amniotic fluid by warm water?

I shall be much pleased and deem it a favour if I hear from you with what success you meet, for it is an important subject, and I feel sure that hot water must soon supersede all drugs and medicated injections, being superior to them all in many important particulars.

1. It is easily attainable at all times.
2. It is absolutely safe, if care be taken to exclude air from the syringe.
3. It stops hemorrhage, not by artifical plugging, but by causing a natural contraction of the uterus.
4. It is cleanly, and a disinfectant, such as carbolic acid, can be easily added.
5. By imparting heat, it rallies the exhausted patient, and gives power to the muscles for contracting, instead of, as is the case with ice, abstracting what little heat remains, and so benumbing and paralysing them.

Can more be required of an agent? It is my practice to have a syringe and hot water always on hand. In case of hemorrhage the water is used as hot as can be borne by the hand. If, however, all goes well, a vaginal bath is given at 100° F., and continued for several days night and morning. A few drops of the strong impure carbolic is added to the water as a disinfectant. This bath always soothes and allays in a marked manner any inflammation and swelling of the external parts. It is a question how high a temperature can be borne, but I have known of a patient using water as hot as 125°.

Let me suggest the use of the hot injection into the uterus for the hastening the removal of the placenta; and, again, if in any case of hemorrhage you should fail to bring on contraction, allow me to suggest the strong tincture of iodine injected well into the fundus to prevent the too early contraction of the cervix, whereby there is risk of some fluid being retained.—*Lancet, June* 22, 1878, *p.* 920.

57.—THE PROPHYLACTIC TREATMENT OF POST-PARTUM HEMORRHAGE.

By Dr. ALEXANDER M'COOK WEIR, Leicester.

Mr. Grose relates an interesting case in the Lancet of November 17th last, in which the subcutaneous injection of ergotine (Bonjean's) was successfully used in post-partum hemorrhage. In a postscript to the article in question will be

found a decided reply to a query of mine published in the Lancet some two years ago. This query had reference to the treatment of hæmoptysis by the subcutaneous injection of ergotine, instead of the roundabout and uncertain methods usually adopted. Mr. Grose is equally positive on the reliable effects of ergotine thus given in uterine hemorrhage and hemorrhages from the alimentary track. I am convinced, however, that there is yet much to be said on the prophylactic treatment of post-partum hemorrhage. No doubt the administration of chloroform had much to do with the extreme uterine inertia, and the temporary removal of the bandage (thus leaving the uterine walls unsupported, so to speak) was the immediate cause of the hemorrhage in Mr. Grose's case.

Latterly, and more especially since reading Dr. Playfair's very excellent book, I have been in the habit of keeping up continuous uterine contraction during the conclusion of the second and until the end of the third stage of labour. This is a good general rule to adopt, and is not difficult to carry out in practice. If the labour be sufficiently advanced, and the pains, from any cause not within the scope of a digital examination, begin to hesitate, and finally disappear, I invariably administer a full dose of Richardson's liq. secalis ammon., which can be repeated in about half an hour, provided one dose has failed to stimulate the uterus into action. This is a beautiful preparation of ergot, and is not disliked by patients; it is void of the *nauseating* effects of other preparations, and has the advantage of being more active and reliable in its operation. It is conveniently given in a cup of tea, and, from the presence of free ammonia, is slightly stimulating beyond its specific effects. Like other preparations of ergot, it will produce "cramp" if given to excess, and before the os is sufficiently dilated—*i.e.*, if given too early in labour. I have given it as a mixture every two or four hours (depending on the urgency of the case) in various miscarriages with undue "loss," and it has seldom failed to expel placental débris, clots, &c., securing contraction, and cutting short the hemorrhage. As I have stated, it does not sicken, and at present I do not know a better therapeutic agent in simple uterine inertia. Therefore, I have no hesitation in pronouncing it a valuable addition to our means of averting and limiting post-partum and other uterine hemorrhages.

But we cannot rely on ergot alone. In order to keep up continuous uterine contraction, the left, or disengaged hand, should be planted firmly on the fundus uteri, which should be followed in its descent as the child clears the lower outlet. As both hands are likely to be engaged at this stage of the labour, the hand of a nurse should be placed in the same position, and

if not frequently reminded she will be pretty certain to relax her hold. No time should be lost in separating the child, and replacing the hand on the partially emptied, and consequently less perfectly supported, uterus. Squeezing should not be immediately practised with a view to expelling the placenta, which, as a rule, will soon separate and be found in the vagina or the bed. This is indicated by a further recession of the fundus uteri, which is now low in the pelvis. I have hitherto omitted to mention "the cord," as I know the eagerness with which the inexperienced and impatient hand will grasp it. I am convinced, after one or two sad experiences, that to drag, pull, or even touch it with a view to hasten the separation of the placenta, is reprehensible and pernicious practice. A uterine squeeze may be necessary to facilitate the separation, but an automatic or voluntary expulsion on the patient's part generally completes the act. We now suppose that the womb is empty and uniformly contracted. The nurse will reach the binder (which should be at least fourteen inches wide, and five feet long), rolled up like a bandage, to facilitate its passage around the patient's loins. When placed in position across the abdomen, the uterine hand must be removed, and the binder quickly carried around, and securely fastened with stout safety pins. Attention should now be given to the pulse, and if about 100 per minute, another dose of ergot should be given, and the result carefully watched. A steady pulse of 80 or 90 after the lapse of an hour need give no alarm. Perfect rest, however, in the recumbent posture for another hour or two is very advisable if there be suspicious symptoms, such as much gaping, rolling of the eyes, and occasional sighing. In this position there is greater command of the uterus, and the abdominal muscles resist less than when the patient is on her side. If there be premature drowsiness, putting the child to the breast is a good corrective and is said to be prophylactic of hemorrhage.

The question now arises—To what extent does ante-partum exhaustion predispose to post-partum hemorrhage? If our knowledge was precise enough, we could no doubt tell the exact time to interfere with the normal course of labour in order to save the mother the consequences of a fruitless task. Another question is—Are we justified in yielding to a patient's entreaties for chloroform when we know that to destroy volition is to efface the earliest and best symptoms of exhaustion? I take for granted that few voluntarily incur the serious responsibility of administering it on any occasion; and as chloroform becomes better known and appreciated as a damper of vital force, a paralyser of the vaso-motor nervous system, and therefore a powerful predisposer to hæmostatic congestion, I think fewer still will sanction its use during parturition. Among the

earlier symptoms of exhaustion may be mentioned general uneasiness, flagging pains, cries for help, a rising pulse and temperature, frequent retching, and a disconcerted countenance. If to these succeed cessation of the pains, sickness, and a pulse of 120, it is time to complete delivery.

I do not know any general rule of time applicable to parturient women. Cases must be judged on their merits, and not by text-books. One can operate earlier if the patient and her friends do not object. Their objections should be overruled if the symptoms are urgent. Speaking for myself, I should advise the patient to forbear the use of an anæsthetic, and, if the os be two-thirds dilated, urge prompt delivery with the forceps. To avoid the shock of delivery completing the exhaustion of the patient it is clear that prompt interference is necessary. Whether the operation be simply to complete an easy labour, or to rescue the mother from prolonged and needless suffering, I think chloroform should be discarded. I shall never forget assisting a powerful gentleman to deliver a monster head with the long forceps. For more than an hour we relieved each other, and at length succeeded with great difficulty, after evacuating the cranial contents. This patient had no chloroform, and, beyond retention of urine for about a week, showed no untoward symptoms. I recall with horror the days in which I revelled in the use of chloroform through ignorance and inexperience, giving it till consciousness had departed, and then resuming the forceps operation!

I delivered a lady (primipara) not long ago with Barnes's long forceps, and took an hour over the operation. The pains had vanished at the end of twelve hours, and could not be brought back again. At the end of eighteen hours symptoms of exhaustion set in, and, although the os was only two-thirds dilated, I determined to operate. Now, I may perhaps be criticised for adopting this course, but, though very modern practice, I think Dr. Playfair, or somebody else, recommends it. It was easy enough to apply the forceps; anything but easy to extract a head above the average size through a firmly solidified pelvis, an imperfectly dilated os uteri, unprepared passages, and over a stoutly resisting perineum. When the head reached the perineum, much care and great patience were requisite to prevent rupture. The tension being great, the pain was considerable, but the free application of lard to the outside facilitated expansion, and the perineum at length "gave way." When I say "gave way," I do not imply that there was a rupture, or even a rent, for there was neither. With the *long* forceps one has perfect command of the labour, and can counteract the final expulsive efforts of the mother. It is only in natural labour, with strong expulsives, that rupture is possi-

ble, if the perineum be not judiciously supported. Of course all that has been said about keeping up uterine contraction applies with double force after an operation, whether it be short and easy or prolonged and difficult. The case related is a fair example of the modern forceps operation, and taking into consideration the extreme sensibility of the patient and the state of the passages, it would be considered a typical case for the exhibition of chloroform. Yet no bad symptoms or untoward hemorrhage followed its disuse, and beyond the retention of urine for a few days (the result of prolonged pressure), there was nothing to alarm the doctor or endanger the patient's life. She left her bed for a few hours daily at the end of a fortnight. —*Lancet, Aug.* 17, 1878, *p.* 214.

58.—ON PLACENTA PRÆVIA, OR UNAVOIDABLE HEMORRHAGE.

By Dr. CHARLES BELL, Edinburgh.

It is obvious that there are two distinct sources of hemorrhage in placenta prævia, namely, the placenta itself and the uterus, and it would be an important point in practice could they be distinguished, as it might lead to more satisfactory treatment. This circumstance, however, will appear more clearly when considering the treatment; but in the meantime it will be interesting to investigate what is the most likely cause of the complication.

Cause.—Various opinions have been entertained on this subject. The older authors, as has been already stated, imagined that when the placenta was found at the lower part of the womb, that it had been displaced by some shock from its original attachment at the fundus; but the opinion which has been most generally entertained in the present day is, that the ovum is not impregnated until it has reached the lower segment of the collum, which is a very likely circumstance, and therefore may be the most frequent cause; but other causes have been assigned, which are well worthy consideration. The one is clearly indicated in Sir Edward Home's interesting paper on the passage of the ovum from the ovarium to the uterus, in which it is stated that the ovum had passed directly into the womb, there having been no apparent decidua covering the entrance of the Fallopian tube, and the ovum was found "concealed among the long fibres of congealable lymph near the cervix." A similar case is reported by Dr. Lee, who informs us "that in the body of a woman who had poisoned herself in the third month of pregnancy, he found both Fallopian tubes pervious, and the ovum being attached by the placenta to the inferior segment of the uterus, it was obvious that it could not have pressed before it the decidua reflexa in the manner usually represented."

This important fact received no particular attention until it was brought prominently forward by Dr. Doherty, in his excellent paper read before the Dublin Obstetrical Society in 1845, in which he says, "The occurrence of full placental presentation, where that substance springs from the whole disk of the mouth of the womb, is, I believe, referable to deficiency in the decidua, which should naturally extend across the orifice of the Fallopian tubes, and the absence, consequently, of the support which ordinarily it is thus enabled to give to the ovum." The frequency with which placenta prævia occurs in some women would lead us to infer that the deficiency of the decidual covering of the orifice of the Fallopian tubes is not always accidental, but may proceed from a natural defect in the uterus which renders it unfit to form decidua at that point.

While deficiency of the decidua may thus be considered a cause of placenta prævia, a like result may probably arise from preternatural delicacy of the decidua, by which it is rendered unable to support the ovum on its arrival in the uterus, or that its weakness induces it to yield easily to any severe shock occurring previous to the ovum being securely attached to the uterus, when it would naturally fall to the more dependent part of the uterus. But these speculations are more interesting in a physiological point of view than practically useful; because, whatever may be the cause of the complication, it cannot be prevented by any human means.

Treatment.—There has hitherto been a remarkable degree of empiricism in the treatment of placenta prævia, arising apparently from its alarming and dangerous character, which has induced some practitioners to endeavour to check the flooding without delay, even at the sacrifice of the child's life. Many remedies have in consequence been adopted, but the first in importance is the artificial delivery of the child by turning. This operation was first suggested by Ambrose Pare, and afterwards strongly advocated by Guillemeau, and it has been considered the most valuable remedy by the generality of the profession since his time, and it is certainly the most advisable when the os uteri is sufficiently dilated, or dilatable to admit of its being performed, more especially if the woman has stamina enough to undergo the operation, and there is an obvious tendency in the uterus to contract. Should there be no evidence of uterine energy, however, it will be necessary to have recourse to stimulants, and the ergot, given either by the mouth or by subcutaneous injection, in order to rouse the uterine energies if possible before attempting the operation. But some accoucheurs have objected to artificial delivery, from its being liable to be followed by fatal consequences. There is too much reason to believe, however, that these results are more frequently pro-

duced by its being injudiciously performed than its inherent character. Nevertheless, the prejudice against it has led to two other operations being suggested as a substitute for it; the one by Sir James Simpson, the other by Dr. Barnes. The operation suggested by Sir James Simpson is the entire separation of the placenta, which he so strenuously advocated, that some practitioners, ignorant of the history of the subject, have supposed that he originated it; but he only revived it, as it was performed by Portal two hundred years ago, and the success attending his operations seems to have induced others more recently to practise it; the most celebrated of whom, previous to Sir James Simpson, was Mr. Kinderwood, who reports several cases, some of which were successful, so far as the mother was concerned; others were fatal to both mother and child. It is very questionable if the cases in which the mothers were saved would not have been equally successful had turning been adopted in place of entire separation of the placenta, when in all probability the child might have been saved.

The argument used by Sir James Simpson in support of this operation is in many instances quite untenable, as it goes on the ground that hemorrhage "chiefly, and in most instances entirely proceeds from the other surface, namely, that of the placenta;" "or, perhaps, more properly speaking, of one large maternal vascular bag, into which the blood of the mother is conveyed by the utero-placental arteries," and by its removal the hemorrhage would cease.

Upon this principle the placenta might be compared to a reservoir supplied by many pipes, and from which, when injured, fluid might escape; but, unless a check were put on the supplying vessels, its mere removal from its locality would not prevent the drain upon the source from which the fluid came; neither will the separation of the placenta check the hemorrhage from the uterus, unless it has energy enough to contract on its vessels, so as to prevent the circulation through them after the placenta is detached. Therefore, if the patient is so exhausted that the uterus cannot act, this operation is equally hazardous to the mother as turning, while it is almost certainly fatal to the child.

Dr. Radford, who seems to be favourable to this operation, says—"I conclude that on a complete separation of the placenta the hemorrhage is immediately and completely suppressed, provided the uterus is in a condition so far to contract as to force down the head with the placenta upon the uterine openings." This is a very erroneous idea, as a little observation will show that the fœtal head is ill adapted to act as a plug; and no uterine pressure would have the effect of suppressing the hemorrhage, which can only be overcome by the same action on

the part of the uterus and its vessels previous to the birth of the child as takes place after delivery.

Dr. Barnes, while he strongly objects to the entire separation of the placenta, advises another operation on the same principle, which has for its object the extension of the partial separation of the placenta, then leaving the case to nature. Now, experience shows that the great cause of anxiety on the part of the accoucheur, and danger to the mother and child, is partial separation of the placenta, in some cases even to a limited extent; yet, this author considers that by this operation "the case is resolved into a natural labour." He founds this remarkable opinion on the supposition that "there is then an anatomical or physiological limit to the extent of placenta liable to detachment during the expansion of the womb;" and that he has discovered that limit, and can discriminate it during labour, and he designates it the "cervical zone," "the region of dangerous attachment," and by separating the placenta from it hemorrhage ceases. This is, however, a mere hypothesis, as there is no part of the uterus from which the placenta can be separated artificially without the danger of hemorrhage, unless uterine contraction immediately takes place. Therefore this operation is equally, if not more, hazardous than the one recommended by Sir James Simpson.

The only tenable argument which has been used in favour of either of these operations is that they can be performed with less shock to the mother, and requires less manipulation, or manual violence as Barnes calls it, than artificial delivery. But this is a mistaken idea. For, in the first place, the os must be dilated to considerable extent before it is possible to introduce the finger sufficiently for the separation of the placenta; and, unless there is great tendency to detachment on the part of the placenta, a considerable degree of force will be required to effect it. This is verified in Dr. Reid's case, formerly referred to, in which he could not force his finger into the anterior part of the uterus to which the placenta adhered; and every one must have experienced the difficulty of separating the placenta in hemorrhage occurring after delivery of the child.

There are other remedies which have been deservedly appreciated in unavoidable hemorrhage, namely, plugging and rupturing the membranes, both of which are most beneficial in the cases suitable for their employment.

Having referred to the most important remedies which have been employed in placenta prævia, it now remains to decide in what cases they are most likely to be useful; and this is the most difficult part the accoucheur has to perform, and his success will, in a great measure, depend on his forming a correct diagnosis.

If the os uteri is small and rigid, this will be rendered a very difficult matter. Therefore our duty will be, in the first place, to have recourse to plugging, until this state of the os is overcome; and the best kind of plug is the indiarubber bag filled with air, which Dr. Keiller had the merit of introducing into midwifery practice. This is infinitely superior to "Dr. Barnes's bags," as they are called, which are filled with water. The bag filled with air not only affords a light and good support, but it enables the accoucheur to ascertain if the hemorrhage is still going on, and it is easily applied; whereas, if a sponge or handkerchief is employed, it is introduced with difficulty, and the blood is prevented escaping, so that the accoucheur is kept in the dark as to the continuance of the hemorrhage, unless the general condition of the patient enlightens him.

If the labour pains are active, it will be desirable to remove the plug to ascertain what progress has been made in the dilatation of the os, and if it is sufficiently dilated, or easily dilatable to admit the hand, and the child has been ascertained to be alive, and the hemorrhage profuse, there ought to be no delay in delivery by means of turning. But if the child is dead, and the mother much exhausted, it may become a question if the entire separation of the placenta may not be attempted, especially if there is a natural tendency to its being detached by the uterine contractions. If the os uteri is not sufficiently dilated to admit of either of these operations, and if the case is one of central presentation, the plug should be again employed, as it is probable that the hemorrhage is caused by the placenta being put on the stretch by the pressure of the child's head, and the support afforded by the plug may have the effect of checking it until labour is further advanced. But if it is a partial presentation, and the distended membranes are found occupying the entire disk of the os, rupturing them may have the effect of checking the hemorrhage, by allowing the uterus to contract on the vessels from which it was flowing, just in the same manner as takes place when they are ruptured in accidental hemorrhage. In regard to Barnes's operation, I cannot imagine any case in which it would be justifiable.—*Edinburgh Medical Journal, June* 1878, *p.* 1088.

59.—COMPLETE INVERSION OF THE UTERUS: REDUCTION ON THE FIFTEENTH DAY.

By ALEXANDER FORD, Esq., L.R.C.P. Edin., Harrogate.

Inversion of the uterus is so rare an accident that the following case may not be without interest.

On February 20th, Mrs. B., aged 22, primipara, was delivered of a healthy child. During the latter months of pregnancy,

she suffered from a fixed pain on the left side; but her general health was good. Labour was normal as regards the first and second stages. After the expulsion of the child, I waited about ten minutes and made traction on the cord, maintaining pressure on the uterus with my left hand. The cord snapped off. I then introduced my hand into the uterus and grasped the placenta, which was partially adherent. I removed all but a small piece. Presently my patient lost a large quantity of blood. She said she "could not see," and felt as though she should "sink through the bed." Her countenance became deadly pale, and her pulse so weak and fluttering that I could not count it. I administered stimulants freely and she rallied. I left her at 11 a.m. I saw her again in the evening and found her very weak. On the following day, the patient complained of having severe afterpains and cramps down the thighs. The bladder had not acted. I drew off her urine, introducing my finger into the vagina to serve as a guide to the orifice of the urethra. There was no change when I saw her in the evening. I passed the catheter, and administered an opiate.

On February 22nd, my patient's condition had altered in that the cramps had subsided; the bladder had not acted. On introducing my finger into the vagina, I came upon a globular body filling up that canal, and this body was the inverted uterus. I made an attempt—a poor one I must admit to reinvert the fundus, and failed.

On February 23rd, Dr. Myrtle saw the patient with me, and rightly judged that reduction would be difficult. Acting on his advice, I consulted Dr. James Braithwaite of Leeds.

On February 25th, the patient was seen by Dr. Braithwaite. His attempts at reduction were unsuccessful. He was, however, of opinion that the end might be attained by tiring out the os uteri, and suggested the exertion of steady pressure on the fundus. The apparatus used for this purpose was an india-rubber pad, fitted to the cup-shaped depression at the extremity of a vulcanite pessary, and maintained in position by a string which was knotted inside an india-rubber ball, and had its other end passed through an annular pad, and through the stem of the pessary. The apparatus was kept *in situ* by two straps, attached behind and in front to a belt passed round the abdomen. The diameter of the cup-shaped end of the pessary was two inches.

I began this plan of treatment on February 26th, and for the first three days I removed the instrument every morning and renewed my attempts at reduction daily. It then occurred to me to keep the apparatus in constantly, and merely undo the straps in order to draw off the urine and syringe out the vagina. I was led to believe this the better mode of procedure, because

the daily removal and reintroduction of the instrument caused the patient much pain, and because the attempts at reduction excited strong expulsive efforts.

March 1st. The pessary was not withdrawn. I contented myself with emptying the bladder and washing out the vagina. I kept up steady pressure on the fundus until the evening of March 6th.

On March 6th, Dr. Braithwaite again saw Mrs. B., and reinverted the uterus while the patient was under the influence of chloroform.

I believe that in Mrs. B.'s case the inversion was not complete until the third day after delivery. The extraction of the placenta caused, I have no doubt, a depression of the fundus, and this depression terminated in the extraordinary accident which is the subject of these remarks. The process of inversion was going on during the day following her confinement; and the cramps were due to the circumstance that four layers of uterine wall occupied the vagina. It was not yet complete; for my finger did not come upon the fundus when emptying the bladder. It was not until about 2 a.m. of February 22nd, that the patient felt a sense of relief from the cessation of the cramps, and on the morning of that day my finger encountered a globular body occupying the vaginal canal. The removal of the placenta was unattended with any difficulty; it did not require as much manipulation as some others which I have had occasion to remove.

Were the symptoms which immediately followed the removal of the after-birth such as to have led me to infer what was the real nature of the case? If so, I did not interpret them aright. They were alarming enough, but not conclusive. Very similar symptoms occur without there being inversion. On January 18th, I attended Mrs. M., aged 34, primipara. Beyond a protracted first stage, there was nothing in the least abnormal in her labour. There was not a single accidental complication, and yet her condition bore a striking resemblance to that of Mrs. B.—*British Medical Journal, June* 1, 1878, *p.* 787.

60.—ON THE EMPLOYMENT OF OXALATE OF CERIUM IN PREGNANT SICKNESS.

By Dr. Francis Edward Image, M.A.

Sir James Simpson introduced the oxalate of cerium, and prescribed it in ten-grain doses. The officinal dose is from one to two grains, which is as a rule so useless that the preparation has been stigmatized as the "oxalate of mud." As a general practitioner of seven years standing, very many cases of pregnant-sickness have naturally come under my care, and up to

the present time I have not met with a case in which the nausea has not been very considerably relieved, and in most cases completely checked by ten-grain doses of the oxalate of cerium. I have at the time of writing this a lady under my care, who from the fourth week of her pregnancy till now, the eighth month, has suffered at intervals from this distressing symptom, but whose sickness has been invariably checked by from two to three days' administration of the oxalate in the dose I have mentioned. In severe cases I give it every four hours for the first day, beginning the first dose half an hour before the patient rises, and then, as improvement takes place, diminishing it to three times a day, but always giving the first dose of the day before the patient moves from the horizontal position—a point to which I attach much importance. The formula I employ is,

℞. Cerii oxalatis, grs. x.; pulv. trag. co., grs. x.; tre. aurantii, ʒ ss.; aquam ad. ʒi. M.f.m.

In Dr. Frowert's case, he prescribed 1½-grain doses, which were "not followed by the slightest remission of symptoms. I hold that this want of good result was from the insufficiency of the dose.

The oxalate of cerium I have also found most efficacious in restraining the nausea resulting from uterine irritation. I generally combine it with bromide of potash in these cases, but have found it succeed in combination where the previous employment of the latter drug by itself has been without appreciable effect.—*Practitioner, June* 1878, *p.* 401.

61.—ON PUERPERAL SEPTICÆMIA; AND ITS RELATIONS, IF ANY, WITH THE POISONS OF THE SPECIFIC ZYMOTIC DISEASES.

By Dr. WILLIAM STRANGE, Senior Physician to the Worcester County Infirmary, &c.

Every obstetrician residing in a large town, and every consultant who has a tolerably wide area of practice, must occasionally encounter one of those appalling cases of (so-called) puerperal fever, which, after taxing all his efforts to baffle the death of its victim, and all his moral courage to support his own and his patient's confidence in the means employed for recovery, oblige him in the great majority of cases, to retire discomfited from the conflict, and disheartened and suspicious of finding success in any future case of a similar character. The very name of the disease, "puerperal fever," is so vague and unscientific as to convict those who use it of vague and indefinite ideas regarding its real nature. That a woman should be occasionally feverish in the puerperal state goes, as the French say,

without telling; but to say that she has puerperal fever tells us nothing of the nature of that fever, of its etiology, pathology, mode of propagation or development. To encounter such a formidable malady with any prospect of success we ought to have the most precise ideas of its true nature; and, further, since experience tells us that, in the great majority of cases, all treatment is of no avail—such profound changes in the vital fluids having taken place as to preclude all hope that the structures can maintain their integrity,—we are thrown back upon merely preventive measures as our only means of safety, the only means whereby we can hope to satisfy the public demand for exemption from the ravages of a malady so unamenable to curative efforts.

It has been my lot to be called in consultation, within a comparatively brief period, to some fifteen or sixteen of these sad cases, three only of which terminated in recovery. Occurring as they do every now and then in series of cases, then being altogether absent for long periods, happening as often, or more often, amongst the higher classes as amongst the lower, the consternation they occasion in provincial society is at once detrimental to the medical man in whose practice they occur, and destructive of public confidence in the resources of our art. You will not wonder, then, that I should have taken pains to convince myself of the true nature of the disease, and will require no apology from me for laying before you the results of my inquiries.

The first point to which I wish to direct your attention is, not the identity or non-identity of the poisons which produce small-pox, scarlatina, diphtheria, or other specific diseases with that which gives rise to true puerperal septicæmia, because a very little examination of such cases on either hand will satisfy you of their entire non-identity, but I wish to inquire whether, in the parturient woman, the poisons of these specific diseases ever by themselves produce puerperal septicæmia of the ordinary character, or are, indeed, capable of doing so.

To answer this question, let us glance for a moment at the symptoms which the poisons of specific zymotic diseases produce both in the parturient and in the non-parturient woman. Now, although I do not affirm that what is called the germ theory of these diseases is sufficient to explain all their phenomena satisfactorily, it is certain that that theory is alone capable of explaining *any* of the symptoms to our satisfaction. The germ theory is thus concisely expressed by Dr. Burdon Sanderson, in those admirable lectures on septicæmia which he delivered at University College at the beginning of the present year. "When a contagious disease is communicated by the atmosphere or by personal intercourse, the conveyance takes

place by means of specially-endowed organisms, which stand in a similar relation to the disease to that which a seed does to a plant. For just as the seed is the origin and the offspring of the plant, so, according to this view, the morbific germ produces the disease, and is in turn produced by it." The term "*contagium vivum*" (living organic matter) which is applied to these organisms, implies a *specific* matter, and relates to specific diseases only. It follows that each kind of these specifically-endowed germs should set up changes in the organism to which they may have been introduced entirely distinct from the changes set up by every other kind of germ; so that the course, duration, and general phenomena of each one of the specific diseases should differ from the phenomena of any other of the class. And so we find it to be. There is no running of one into the other, for in every instance these germs are observed to *breed true*.

A second division of the germ theory, and one which has given rise to much more discussion and difference of opinion than that just named, holds that the atmosphere, especially the atmosphere of hospitals and crowded dwellings, teems with organic particles, particles of extreme minuteness and tenuity, —organisms, indeed, but whose existence, in some cases, is rather arrived at by induction than proved by observation, for they are not recognisable by our senses even when aided by the most powerful microscopes. Professor Tyndall has succeeded in isolating them, and when sown upon a suitable soil, such as decomposed blood or disordered secreting organs, produce all the phenomena of the most fatal septicæmia. "It is these organisms," says Dr. Sanderson, "which, entering open wounds or breaches of surface, are the cause of suppuration and of those other destructive changes which interfere with the healing of the wound, whilst, on the other hand, if these particles be absolutely excluded, all putrefaction of an otherwise septic fluid is prevented."

Now it is but a short step from the open wound to the condition of the uterus and its passages in a parturient woman. Taking the open uterine sinuses as so many open wounds, what is to hinder these septic particles of the atmosphere from gaining access to them, and so setting up the same series of changes in this case as in the other, thus making puerperal septicæmia twin sister to surgical pyæmia? And if this deduction be accurate, may we not hope to find a means of excluding the entrance of these germs into the system of the lying-in woman by some such method as that so successfully employed in preventing the contamination of surgical wounds by them, and, by so doing, exempt the patient from the horrors of septicæmia altogether?

We must reply to this question, however, very doubtfully, at least at present. The experiments of Professor Tyndall, M. Pasteur, Dr. W. Roberts, and others, show that it is with the very greatest difficulty that their prepared fluids can be kept free from the contamination of septic germs, which not only float in the atmosphere, but may adhere to every instrument and to every vessel used, as well as to the clothes and hands of the operator. Even boiling water will not always kill them, nor any filter, short of a porcelain one, obstruct them. This being so, how can we hope that these germs will not occasionally be carried in the clothes or by the hands of the obstetrician, of the nurse or other attendant; and if these persons be in the habit of frequenting hospitals, the bedside of sick persons, or others in whose secretions these germs abound, the only wonder is that, instead of being a comparatively rare disease, puerperal septicæmia is not of everyday occurrence. One thing, however, is clear, to my mind at least, that these septic particles, whether introduced to an open wound or into the open uterine sinuses of a parturient woman, never give rise to anything resembling the effects of the poisons of the specific diseases; and therefore that their action upon the system is *sui generis*, altogether as distinct as that of each of the zymotic diseases themselves. As small-pox never becomes scarlatina, or scarlatina measles, so none of these can ever become true septicæmia, such as we see it in puerperal fever or surgical pyæmia.

That these deductions from observed facts are important everyone will admit, but that they are universally or even generally admitted is disproved by what we hear and see almost every day. It has occurred to me on several occasions to hear men, and men of eminence, too, whom I have met in several cases of puerperal septicæmia, ask if the patient has been exposed to the poison of any of the zymotic diseases. Had the accoucheur or the nurse been lately in attendance upon a case of scarlatina or of measles, &c. ? Or, has the patient herself, just previous to her confinement, been in some way brought into contact with the contagion of some of these diseases ? You cast about, and are probably unable to guess at the possibility, or, at least, at the probability, of such a chance. Or, on the other hand, the unlucky practitioner or nurse gets blamed for a proceeding which, after all, may have had nothing whatever to do with the causation of the disaster.

But suppose that the patient have palpably been exposed to the contagion of, say, scarlet fever, just prior to her lying-in, what are the symptoms we find ? In most cases, we find, a few days or hours after delivery, the patient terribly ill, with a hot skin, generally preceded by rigors, rapid pulse and perspiration,

urine high-coloured and scanty, the head painful, the mind clouded, with a tendency to coma, leading in most cases to speedy death. The milk and lochia have been suppressed from the beginning. And all this passes in three or four days. Now, can we discriminate whether the case be one of puerperal scarlet fever, measles, or small-pox? Except in a very few cases, where death supervenes almost immediately from the profound alteration in the blood, caused by a great dose of the poison, we can. Some of the distinctive features of each of the "zymotics" will be almost surely discernible, such as dusky skin and ulcerated fauces in scarlatina, raised papules in small-pox, and the characteristic blotches in measles. Moreover, cases of zymotic disease usually run the same course in the puerperal state, if the patient survive, as they do in the ordinary patient. Any one of the specific diseases may be contracted by a non-parturient from a parturient person, while there is no instance of any infection passing from a genuine case of puerperal septicæmia to any other person than one situated like the patient.

I shall prove presently, after adverting to the characteristic features of true puerperal septicæmia, that there is no analogy, save one of the very loosest kind, between the symptoms and the pathological history of the ordinary zymotic diseases and those of septicæmia as it affects the parturient woman. But some still sceptical person may ask why it is that we so often find that scarlet fever, or some other disease of its class, breaks out just at the time of parturition, or a few days after, if it be not connected in some way with the peculiar condition of the parturient woman—transformed, in fact, into puerperal fever? I believe the following to be the explanation:—If the patient have been exposed to the poison of some one of the zymotic diseases a few days prior to her delivery, whether the delivery be at term or not, her delivery is determined at that particular time by the reaction of the poison upon her blood, and then the more obvious symptoms, such as the rash, &c., follow in their natural course just after delivery, modification, but not change of character, being the result.

I would here draw a caution from these facts, which may be of the greatest service to us. It is, that, whilst casting about to discover in what way the patient suffering from puerperal fever may have picked up the poison of some contagious disease, to take care that we are not neglecting the true cause of the symptoms, and letting pass the precious moments in which alone the mischief can be arrested, by removal of all that we can of the poison already *at* her person, and by preventing any more gaining access to her. In fact, whilst our minds are occupied with the notion that one kind of poison *may* have got

access to her blood, do not let us neglect to remove the septic poison already lodged in her uterus or vagina, or the germs which are gaining entrance there from the atmosphere, or by the vessels or hands used by the practitioner or the nurse.

Let us contrast the symptoms of genuine puerperal septicæmia with those of the specific contagious diseases when they affect a parturient woman. In the former case rigors are seldom present at the first, at least to any extent. The temperature may run high, but the breathing is not hurried. The skin preserves its colour, or else assumes an ashen whiteness. The head remains clear and painless, and there is an absence of all alarm with respect to the issue of her illness. The milk and the lochia are suppressed after some days, but much more slowly than in the case of the specific diseases. Whilst cases of these latter nearly always run on to coma and death in two or three days, the sufferer from true septicæmia may linger on for days, and even for weeks, dying at length from ulterior changes in the blood and nervous system.

A few words upon the nature and action of this septic process, which, introduced into an open wound, may produce pyæmia, or, into the uterine sinuses of a parturient woman, septicæmia; but which, both in its mode of origin and in the effects which it produces, is totally distinct, in my opinion, from that caused by the poison germs of any of the specific diseases. "It is easy," says Dr. Sanderson, "to prepare a putrid infusion of muscle possessing such toxic properties that *less than half a grain* of it introduced into the blood of a dog will produce death." So also, "a single drop of the exudative fluid of some inflammatory disorders of the serous membranes will equally kill if injected into the veins or under the skin of an animal." Now, I take it as proved that it is the growth of bacteria in these fluids which makes them so deadly. And not alone the *growth* of bacteria, for they by themselves are harmless enough, since they enter the blood continually in thousands without producing any ill effects; but the bacteria, when planted in a suitable medium, such as decomposed blood or the products of inflammation, produce the septic poison, it may be by fermentation, it may be by secretion; and thus, although bacteria are of themselves innocent, there can be no septicæmia where they are not or have not lately been present. And these are the symptoms to which the injection of such septic matters gives rise: shivering or shuddering; restlessness, muscular debility, and unsteadiness of gait; then vomiting, purging, tenesmus, and alvine irritation. There is also some rise of temperature at first. If the dose has been sufficient—*for everything depends upon the quantity of the septic poison which has been introduced*—death speedily follows these symptoms. The post-

mortem appearances in such cases are (quoting from Dr. Sanderson): "Blood extravasated underneath the pericardium, sometimes in the neighbourhood of the valves, and chiefly on the left side. The pericardial and pleural surfaces are ecchymosed. The mucous membranes of the stomach and intestines are invariably found of a red colour, from the detachment of epithelium; the spleen is enlarged, and the abdominal organs generally are in a state of congestion. Now, the essence of all this is capillary congestion and hemorrhage of the abdominal viscera generally, leading to blood stasis and shedding of epithelium. Microscopically, the blood-globules are found dissolved in the serum, the colourless corpuscles being also disintegrated."

Now the phenomena just enumerated, whether of the symptoms during life or of the appearances after death, very nearly resemble those which are met with in surgical septicæmia; and those of puerperal septicæmia are essentially the same. As Dr. Sanderson says, "considering that the anatomical conditions present in the puerperal state are in themselves so favourable to the entrance of septic matter, we should expect to find in obstetric practice cases more comparable with experimental septicæmia than in surgery. Puerperal septicæmia must therefore be regarded not so much as a disease as a complication, differing from pyæmia not only in the fact that it has not necessarily a connexion, although generally it has, with any local morbid process, either primary or secondary, but also in the important particular that it has no development. Pyæmia is a malignant process which goes on to its fatal end; but in the case of septicæmia, inasmuch as the poison which produces it has no tendency to multiply in the organism, there is no reason why the morbid process should not come to an end of itself, unless either the original dose be fatal, or a *second* infection from the same or from another source take place."

It would appear, then, that bacteria, which are everywhere present, require a suitable *nidus* in which to operate in order to produce their septic effects. That *nidus* is presented by any wound or open part of the body which contains unhealthy secretions or disintegrated blood or tissue. The open uterine sinuses, the abraded mucous membranes of the vagina or vulva, of women, after delivery, present the situation of all others most favourable for these septic operations, provided there be blood or secretions of an unhealthy character to be operated upon, *and a state of the system which is ready to be easily affected by the process.* For it has been proved experimentally—first, that broken-up blood-globules, introduced into the circulation of a dog, produce all the phenomena and fatal effects of septicæmia immediately; secondly, that if you allow blood to

coagulate and then strain the serum off the clot, this serum, injected into the veins, also acts as a poison; the source of mischief in this latter case being the disintegrated leucocytes or white corpuscles of the blood.

It follows as a matter of absolute necessity, in view of these facts, that all clots, altered serum, or secretions should be most assiduously washed away from the uterus and passages of a parturient woman, whenever we have reason to fear that this infective process is being set up. We must act upon the hope that, although *some* poison may have entered the circulation, that portion may not be sufficient to kill the patient. To remove from her person any more of it that may be lying ready for absorption must be the first object; whilst, at the same time, we may try to counteract the effects upon her system of that which has already been absorbed. That poison, as I have said, may be lurking in the vagina, or in the uterus itself; therefore, to be effectual, syringing must be carried right up to the uterine sinuses. This, in such cases, should be done almost hourly, so as to anticipate, if possible, the absorption of more septic matter. No hand but that of the accoucheur can do this effectually; and no man can feel comfortable who, seeing septic infection going on, does not himself grapple with all his might with so deadly an enemy.

To sum up my argument, then, I reckon that there are three ways in which a parturient woman may suffer special damage:—

1st. One of the specific zymotic diseases may attack her immediately before or after her delivery. The result will be a modified and generally a very severe or fatal form of that same disease, which will run the usual course with or without other complications. The germs of these diseases, as is well known, multiply in the system, and speedily affect the whole of it. This poison may be communicated to any number of non-parturient and healthy persons.

2nd. By the introduction of a septic poison from without, as from the atmosphere of the lying-in room, or from that of a foul neighbourhood; or it may be conveyed to the patient in the clothes or on the hands of the practitioner, the nurse, or other attendant. This septic poison cannot be that of one of the specific diseases, for we have seen that *they always breed true*. Moreover, it must be a poison containing bacteria, because without bacteria there can be no septicæmia. That it can be carried about from place to place without giving rise to any suspicion of its presence is accounted for by its extreme minuteness and tenuity.

3rd. The septic poison may be generated in the body of the parturient woman herself, on the introduction of (harmless) bac-

teria or other germs from without. We have seen that decomposed blood or disordered tissue or secretion forms the favourite *nidus* of bacteria to act upon, so as to produce their poison, whether in a surgical wound or in the open sinuses of a recently delivered woman. As the atmosphere, especially that of hospitals and sick-rooms, contains innumerable germs, which only require to be sown upon a suitable bed to acquire their poisonous properties, what wonder that they sometimes gain access to such a bed as the bruised uterus or vagina presents! The only wonder is that, upon this theory, not a few only, but the great majority of parturient women do not fall victims to septicæmia. The explanation is well pointed to by Dr. Sanderson. To become fruitful for mischief, these germs require unhealthy blood or tissues to act upon, or a system in a condition (unknown) ready to absorb the products of such union. Into the healthy blood and tissues bacterial germs enter in numbers continually; but as fast as they enter they are destroyed by some destructive (colytic) action of the healthy blood and tissues.

The septic poison, once generated as above described, spreads from tissue to tissue, sometimes rapidly, at other times more slowly, affecting now one, now another of them; in this respect resembling a cachexia, such as tubercle or cancer, rather than the specific poisons which multiply themselves rapidly in the system, and, after a few days, are all cast out.

The appearances after death from puerperal septicæmia have been described as strictly resembling those which have been observed after the injection of a septic poison into the blood of an animal. They are, shortly—capillary congestion of various organs, particularly of the mucous membranes of the stomach and intestines; infarction of bloodvessels in the lungs, liver, &c.; and, towards the end, evidence of embola in the brain and nervous centres; and when we remember the ease with which the disorganised blood in the uterine sinuses can enter the circulation after labour, it is difficult not to believe that in all this we have the *fons et origo* of most of the cases of puerperal septicæmia.

The symptoms, also, are those which flow naturally from some such starting-point: loss of muscular strength, so characteristic of every case of the disease; breathlessness, palpitation, quickened respiration and pulse; rise of temperature, varying but little between night and morning; then disturbance of the stomach, vomiting, diarrhœa, with a clean red tongue, from loss of the epithelium. As regards the nervous system, there is generally clearness of intellect, with complete unconsciousness of the gravity of the illness. Food is taken tolerably well for some time. Towards the end the intellect may become clouded, with total apathy to all surrounding objects.

I feel that I should have been only wasting your time were I to conclude without drawing some practical conclusions from the facts and arguments stated above. 1st. We ought, as a matter of course, to be most careful to guard an expecting parturient woman from every chance of infection from any of the specific contagious diseases, and to use the most scrupulous care to prevent ourselves from being the carriers of such infection. 2ndly. We ought at the same time to be on our guard lest the idea of this class of disease should either warp our judgment or blind us to the probable fact that the case before us is one of *true* septicæmia, and owes its origin to quite another form of poison to that of the contagious diseases—viz., to the importation of the poison generated by bacteria from some blood disease, or cachexia, in which these organisms abound. Lastly we must take care that the atmosphere and other surroundings of the patient are kept so as to eliminate all suspicion of infection from them.

But it may be asked, How would you treat a case in which symptoms of septic poisoning have already set in? First of all, I would act upon the hope that, although some poison has been absorbed, that portion may not be sufficient to kill the patient. Remove from her person, by repeated syringing up to the uterine sinuses with disinfectants, any more of the poisonous stuff which may be lying there ready to be absorbed. Counteract as far as possible the effects of that which has been already absorbed, mainly by supporting the strength of the patient by mild nourishing food and *moderate* stimulants.

Then as to disinfectants. What a farce does it not seem to us to see nurses and attendants placing a little Condy's fluid in saucers about the room of a woman lying ill of puerperal septicæmia! The disinfectants are wanted in the vagina and uterus—even in the blood itself; and all other modes of using them are but a delusion and a snare, which may result in the most fatal consequences. The great desideratum undoubtedly is some substance which, introduced into the patient's blood, would kill the poisonous germs without also killing the blood itself. Such a substance is not yet known, but there is no reason, theoretically speaking, why it may not be discovered. Salicylic acid, or the new disinfectants thymol and carvol, should have a fair trial; and we may hope that some other germicide may be discovered which, whilst tolerated by the blood, may be destructive of the poison in it. Meantime, as I set out with observing, until the happy discovery of some such *curative*, we must pay all the more attention to *prevention*; and I feel assured that in the acceptance of the theory of septic poisoning which I have endeavoured to illustrate from the experiments of the best observers you will find a rational course of action;

whilst, at the same time, your responsibility will be enormously increased if you neglect to take the most minute precautions against so terrible a malady.—*Lancet, July* 20 and 27, 1878, *pp.* 79, 115.

62.—ON PUERPERAL FEVER.

From the Report prepared by the Puerperal Fever Committee of the Berlin Obstetrical Society.

Puerperal fever must be looked upon as, in the fullest sense of the words, a great national calamity, and we anxiously and expressly put the question, "Can it be avoided, and how?"

Before we proceed to answer this question, allow us to give a broad outline of what we mean by puerperal fever, and how it is brought about.

Under the names "puerperal fever," "malignant childbed fever," are included a group of diseases occurring in childbed, which vary very greatly in their manifestations, but have this in common, that they are called into being by the absorption from the organs of generation of a material which gives rise to destructive inflammation and fever. There are, indeed, a number of substances, mainly composed of organic materials in a state of putrid decomposition, which, when brought into contact with an open wound, set up inflammation in it, which extends to the neighbouring tissues; a further absorption by the lymphatics and bloodvessels leads to more extensive inflammation among neighbouring and remote organs; and when a large quantity is rapidly absorbed into the blood, a quickly fatal poisoning of the whole organism occurs. To surgeons the deadly effect of these materials upon wounds is only too well known, and the greatest advance, probably, which surgery has ever made consists in the so-called antiseptic method of treating wounds—that is, in the scrupulously exact removal of such materials from fresh wounds.

Puerperal fever is indeed nothing else than the infecting of fresh wounds, such as are found in every newly-delivered woman, with these destructive septic materials. Almost every woman after labour has small wounds on the external genital organs, which are caused by the passage of the child through this narrow opening, and in every newly-delivered woman the inner surface of the uterus, from which the protecting membrane has been cast off with the ovum, presents a large wound surface. Thus every newly-delivered woman is liable to suffer from the dreaded infective wound diseases,—which in persons wounded under other circumstances are called pyæmia, septicæmia, wound fever, bloodpoisoning, purulent infection, &c.,—*so soon as suitable septic materials are brought into contact with the genital organs.*

Now, materials of this sort gain admission in two ways: first, and this happens more especially in very difficult and long labours, under the influence of the particles that cause putrefaction which are ever present in the air and ever ready to press in, decomposition occurs in the mother's own secretions and excretions, and thus takes its rise in the maternal organism itself; or, secondly, these materials are introduced into the female genital canal *from outside*. This latter is brought about almost exclusively by the finger or instruments of those who examine the lying-in woman—that is to say, of the midwife or the physician.

If the instruments or finger of these persons have not been cleaned with the greatest care, and disinfected most conscientiously after they have been in contact with any infective matters, the result is that these matters are brought into contact with the fresh wounds of the woman during labour or subsequently, and thereby infect her with a fatal disease. A specially frequent channel of infection is from a diseased lying-in woman, to another, because midwives quite commonly go from a diseased lying-in woman, whom they are nursing, to attend upon a new confinement, without sufficiently purifying themselves; and it cannot be doubted that, in the majority of cases, the midwives are the carriers of this infection, because, to their exclusive care the great majority of labours is intrusted, and they naturally come into more intimate relations with the lying-in women than the physicians, who, in very many cases, are only called in after the onset of threatening symptoms.

From the foregoing exposition it will be seen that we know the cause of puerperal fever, and the manner in which it is brought about, more accurately than in almost any other disease, and, on the strength of this knowledge, have to ask ourselves the question, By what regulations can the occurrence and spread of puerperal fever be prevented, or at least lessened in amount?

These regulations naturally divide themselves into those which aim at preventing the occurrence of individual sporadic cases of puerperal fever, and those whose object is to avoid the transmission of the disease from infected lying-in women to healthy ones.

As regards the first object, it is obviously of great importance to avoid the putrid decomposition of the discharges which come from the woman during labour. This is, of course, the aim of a rational management of a case of labour, which would, if possible, bring the labour to a conclusion before any stinking excretions come from the genital parts; we work in the same direction by keeping away anything which can excite putrefaction. The midwife should, accordingly, remember that frequent

and careless examinations hasten the decomposition of the secretions, and should be instructed to use *disinfectant injections* in all cases of prolonged labour, so that the products of decomposition may be formed in the female genital passages as late, and in as small quantity, as possible. Of still greater moment is it that the medical persons (*Medicinal-personen*) to whom is entrusted the care and treatment of the lying-in woman, should recognise to its fullest extent the danger which threatens women, where putrid substances are brought into contact with the sexual parts either by the finger or by instruments. Among physicians this danger has long been known, and is universally recognised; but midwives cannot be too earnestly and strongly warned of it. That the rising generation of midwives may be properly instructed in this matter, will require even more careful attention in the new editions of the hand-books for midwives, and in oral instruction, than it has hitherto met with; but it is quite essential that those already in practice, that is, those who were passed midwives prior to 1866, the year in which the present *Kangow Hand-book for Midwives* was introduced, should receive fundamental and exact instruction as to wherein the danger lies, and how it is to be avoided. We do not consider it as our object to work out such a system of instruction, but we feel it our duty to lay great stress upon this point, because the great majority of our practising midwives are without anything like a sufficient appreciation of the danger.

Indeed, since it may be long ere all midwives are thoroughly instructed on this subject, the question arises, Whether it would not be the wiser plan to appeal directly to the public in this matter, and to enjoin upon husbands not to allow their wives to be examined by any hand which has not previously been thoroughly disinfected?

When we reach this point, when it is the universal custom, *in every single case, and under all circumstances, for the physician and midwife to disinfect their hands before they introduce them into the genital organs*, then, without any doubt, the annual mortality in childbed will become very much less, and thousands of women, who now die of infection brought about by thoughtlessness or ignorance, will be saved.

It remains to speak of special rules for those cases in which puerperal fever has already broken out, because the danger of transmission of the disease then becomes more considerable, and experience teaches us that, where definite epidemics of puerperal fever occur, *they cling for the most part to the practice of a single midwife*. A large number of instances are on record in which a midwife has carried the infecting material from one diseased lying-in woman to others, with the result that a number of

newly-delivered women have sickened, and many of them have died.

In order to cut short these definite epidemics when they arise, and to prevent them from breaking out, it appears absolutely necessary *to lay upon all medical persons* (Medicinal-personen) *by law the duty of reporting such cases to the Sanitary Boards*, The simplest way of effecting this is to include puerperal fever among the contagious diseases, of which it is necessary to send in a report. Undoubtedly special difficulties present themselves in regard to diagnosis. The question whether a disease during lying-in is to be looked upon as an infectious childbed fever or not, may be a very difficult one even for the physician, and is entirely beyond the judgment of a midwife. Accordingly, we hold it to be the bounden duty of midwife and physician to make a report to the sanitary authorities, *in every case of severe feverish disease occurring in childbed, unless it be clearly established that it has no connexion with the puerperal process.*

But since even here differences of opinion may arise, there is one thing, at all events, about which there can be no doubt, namely death; we therefore deem it necessary to add, *that all midwives are in duty bound to give notice of every fatal case during childbed which occurs in their practice.* The sanitary authorities will probably in this way get sufficiently early notice of the existence of puerperal fever epidemics.

But if they are to be in the position effectually and certainly to cut short commencing epidemics, *they must have the power to suspend the midwife from the practice of her calling for a fixed period*, since this is the only sure means of preventing the extension of the disease through the same midwife.—*Edinburgh Medical Journal, Nov.* 1878, *p.* 443.

63.—ON THE MODE OF EXAMINING PATIENTS WITH ABDOMINAL TUMOURS.

By T. Spencer Wells, Esq., Hunterian Professor of Surgery and Pathology to the Royal College of Surgeons.

[The five following articles are abstracts of Prof. Wells's lectures on the "Diagnosis and Surgical Treatment of Ovarian Tumours," delivered at the Royal College of Surgeons.]

The lecturer entered at considerable length into the mode of examining patients with abdominal tumours, describing in detail the methods of external, internal, and combined examination, and showed his form of note-book for recording cases. He described the mode of distinguishing collections of fluid in the abdominal cavity from collections in cysts, and illustrated, from preparations in the museum, ovarian, renal, and hydatid cysts.

We give the following remarks on combined internal and external examination of the abdomen and pelvis:—

"With the thumb in the rectum and the forefinger in the vagina we can often get an accurate notion of what may be contained in Douglas's pouch; or, on the other hand, if the thumb is on the cervix uteri and the forefinger in the rectum, it is quite easy to feel a considerable part of the uterus, even to the fundus, and so get a notion of its size and form, or of anything attached to its exterior, either in front, behind, or at the fundus.

"Simon, of Heidelberg, laid great stress on the combined examination of the bladder and uterus after dilatation of the urethra, believing that this was not only useful in completing diagnosis of disease of the bladder itself, but also for examining growths in the vesico-uterine pouch, tumours on the anterior surface of the uterus, or on either side of the pelvis, where they extend forwards. Combined examination between the walls of the abdomen and the bladder may occasionally become necessary. In some forms of uterine disease combined examination may be assisted by previous dilatation of the neck of the uterus with a sponge tent; and in other cases, where examination by rectum alone, or combination of rectal and external examination, may be insufficient, as in inversion of the uterus or congenital absence of this organ, combined examination by bladder and rectum, either by finger in rectum or sound in the bladder, or finger in bladder after dilatation of urethra, gives all the information required; but this seldom can be necessary, except in cases of atresia of the vagina.

"As Hegar has pointed out, if the thumb of one hand in the vagina fixes the vaginal portion of the cervix uteri, the index-finger of the same hand in the rectum can not only feel the posterior surface of the uterus distinctly, but can follow the sacro-uterine ligaments; while, if the other hand presses the abdominal wall backwards towards the sacrum, a very accurate idea can be obtained of the relations of all the pelvic organs. The uterus can be moved in various directions, and anything between it and the bladder or rectum is distinctly felt, supposing of course no extraordinary amount of fat in the abdominal wall, nor any peculiar rigidity in the vagina, interfere. Flexions of the uterus are thus very accurately recognised, and often replaced easily.

"These examinations must be carried on, sometimes with the patient on her back, sometimes on her side, and sometimes in both positions, and occasionally in the knee-and-elbow position, with the shoulders low, a change of position of the organs giving information otherwise unattainable.

"Simon lays great stress on the fact that when a patient is

deeply narcotised the whole hand may be passed into the rectum. I have done this occasionally, but have not obtained much additional information than is given by one or two fingers.

"Hegar deserves the credit of introducing a method of examination which, in some cases, is really of very great value. He fixes the vaginal portion of the cervix uteri by a pair of long hooked forceps, by which the uterus may be drawn downwards or on either side. The same object may be obtained more safely by one of Marion Sims's hooks, and there can be no better method of clearing up doubts about the size and position of the uterus, its connexion with neighbouring organs, and especially its relation with abdominal and pelvic tumours.

"I need not say that this must all be done with due care; that no forcible traction upon the uterus must be exercised, and that steadying the organ will often be found quite enough.

"Suppose the uterus thus fixed and gentle traction made upon it with one hand, and one or two fingers of the other hand are passed into the rectum, the posterior surface and sides of uterus are felt, and, if necessary, the finger may be carried over the fundus. Sometimes the forceps or hook may be given to an assistant, while one or two fingers of one hand in the rectum and the other on the abdominal wall effect a combined examination of the most complete character. The connexion of the abdominal tumours with the pelvic organs may be very accurately made out. A slight pull on the uterus may be sufficient to clear up any doubts as to the connexion between the uterus and the tumour, while the pedicle or membranous adhesions with the rectum may be made tense and felt.

"Supposing a tumour is partially or entirely in the pelvis, in more or less close apposition with the uterus, by drawing the uterus downwards or forwards on to one or other side, the examining fingers in the rectum may follow the outlines of the tumour and notice how its movements are affected by the movements of the uterus, or if it may be separated from the uterus. It is by no means unfrequent that you can separate the uterus from a tumour where previously there had seemed to be intimate connexion, or union apparently inseparable. The assistant drawing down the uterus or to one side, with two fingers in the rectum and the other hand over the abdomen, pushing up the tumour, we may often get an idea of the length of the pedicle, and in reference to uterine fibroids information as to the possibility of removing them. You find out the length and thickness of the cervix, whether it is fixed or movable, and whether it is involved in the new growth. You pull, as it were, the neck of the uterus out of the mass which in a measure involved it, and this shows the tumour to be a growth which may be removed." —*Lancet, June* 22, 1878, *p.* 918.

64.—ON THE DIAGNOSIS OF OVARIAN DROPSY.

By T. SPENCER WELLS, Esq., F.R.C.S. &c.

I will suppose we have a patient who has a collection of fluid, either free in the abdominal cavity, or contained in some cyst. [Two diagrams were here shown, representing the abdomen of a patient containing fluid.] We will suppose the central dark shade to represent fluid, and that the rest of the surface is resonant on percussion. The part of the abdomen which contains the fluid is dull on percussion. In this other case, the dark shade shows dulness on percussion on both sides, the patient lying on her back; the centre is resonant on percussion, but the sides are dull. In the first case, the central part is dull on percussion, and the sides are quite clear. The inference would be at once that, with intestines at the sides and the fluid in the centre, the woman lying on her back, the fluid must be enclosed in a cyst. On the other hand, here the intestines are floating, giving a clear sound on percussion, and the fluid gravitates to the most depending parts of the cavity, and you will see at once the fluid must be free; the intestines are floating about and the fluid is beneath. As a rule, I suppose, in nine cases out of ten, that would probably be the correct diagnosis. You would say here was a case of ascites, and here, where the intestines are pressed backwards and the fluid is in the centre, that will be encysted fluid; and, in a large number of cases, that will be certainly true. But there will be exceptions. There may be so much fluid in the peritoneal cavity that the intestines do not touch the front of the abdomen, or the intestines may be fastened backwards by adhesions; and fluid which appears to be in front and is in front, is there, not because it is enclosed in a cyst, but because the intestines are held back. And, on the contrary, one may get a case where fluid is enclosed in the front by coils of intestine round it, and is circumscribed in that manner.

I have had a number of preparations brought down to show the way in which adhesions of different portions of the intestine to each other, and to the abdominal wall, may simulate cysts, may become false cysts. The fluid is encapsuled, not by a cyst, but by surrounding portions of intestine or omentum adhering to each other, or to some part of the abdominal wall. Those are our most puzzling cases in diagnosis at times. In the book on "Diseases of the Ovary," which I published some years ago, there is rather a full account of all this, with which I need not trouble you now. I ought, however, to have mentioned that this fluid, when it is free, will move from side to side as the patient is moved. If she is turned on to her left side, the intestines go to the right; and when she is turned back again,

the fluid sinks to the right side, and the intestines float up to the left. So when she is sitting up more or less, and her shoulders raised, the fluid sinks down towards the lower portion of the abdomen, leaving the upper free. If you put a pillow under the hips and lower the shoulders, it at once goes upwards towards the sternum. So, occasionally, one may be deceived by a cyst of moderate size sinking, if it is movable, from side to side as the patient is moved.

The alteration of the position of the patient makes considerably greater and more rapid changes in cases of ascites than in patients with ovarian cysts; the cyst moves about much more slowly than fluid. The superficial veins are more commonly enlarged in ascites than in ovarian dropsy, and, on measurement, the enlargement of the abdomen in ascites is more likely to be symmetrical than in the ovarian cyst; and then, on feeling the abdomen, you are more likely to get a flaccid abdominal wall in ascites, and a more tense wall in a cyst; and very often, in ascites, the greatest girth is at the umbilical level; whereas, in ovarian dropsy, it is sometimes considerably lower.

The way in which the fluctuation is felt is of some importance in ascites, the wave being a wave felt more immediately beneath the hand, and not limited so distinctly by the cyst as in ovarian dropsy. Then, a very important question is the way in which the movements of respiration affect the dulness. Supposing one finds that, as the patient breathes deeply, resonance descends, it will often guide you correctly in a doubtful case, by a careful examination, as to how far the line of clearness on percussion varies or corresponds with the line of fluctuation. When you get distinct fluctuation beyond the limit of dulness—when you get evidently a wave of fluid moving about amongst the intestines—it is pretty certain that the fluid is free. However, it may be limited; if one finds one has a distinct wave fluctuation where one has a resonance indicating the presence of intestine, that must always certainly be free peritoneal fluid, and it is the same whether it is ascitic fluid or fluid escaped from a cyst. The fluid is free, with the intestines floating about. In the other case, you feel the wave of fluctuation limited exactly by the outline of the dulness. One can very seldom go wrong by carefully observing these two points.

I have said just now a doubt may arise where the quantity of fluid free in the peritoneal cavity is so very large that the front of the abdomen is pushed forward far beyond the reach of the intestines, the intestine cannot reach the surface of the abdominal wall, and so one has the fluid in front.

Another point of doubt and difficulty may arise from the air having entered an ovarian cyst; for, as an ovarian cyst contains a certain amount of fluid, it may also contain gas, and either

the fluid is decomposed after tapping, or there may have been some possible communication set up between the intestine and the interior of the cyst. Sir Thomas Watson records a case in which a patient had a cyst filled alternately with fluid and witb air; when the fluid collected to a certain quantity, it seemed to open some valve-like communication with the intestine, emptied itself, and became filled with air. As the fluid gradually re-formed, the air was displaced, and the same series of changes went through again. I have known a case in which air distinctly entered an ovarian cyst through the Fallopian tube, at least, we could find no other means of entrance; and, after death, it was found there was a large and distinct communication between the interior of a large ovarian cyst and the uterus through an enlarged Fallopian tube; there would have been, at any rate, no difficulty in air entering in that way.

Supposing doubt does arise as to whether fluid is free or not, one has always the resource of tapping; and a chemical and microscopical examination of the fluid that is removed will settle any doubt as to whether it is free peritoneal or ovarian fluid, or fluid of some other cyst. Years ago, before Scherer's test to distinguish albumen in its true form from the forms of albumen that chemists do not seem very accurately to determine, it was sometimes extremely doubtful to arrive easily at this determination. But Scherer showed that the albumen which, when coagulated by heat, is redissolved by acetic acid is a very different albumen from true albumen. Paralbumen or metalbumen are forms that, when coagulated by heat, can be dissolved by acetic acid boiled. One takes a test-tube and boils the fluid, and the albumen is coagulated. You add a little strong acetic acid, and give it a good boil, and shake it up; and if the albumen be true, the coagulum does not redissolve in the acetic acid. But supposing it to be paralbumen, then it either dissolves, or forms a whitish transparent fluid, or breaks up into a kind of jelly-like transparent mass, which is quite easily distinguishable from undissolved albumen coagulated by heat. This was supposed to be, for a long time, a pretty certain means of distinguishing between ascitic fluid and ovarian fluid. If the coagulated albumen were dissolved in acetic acid, it was said to be ovarian, and if it did not redissolve it was said to be ascitic; and that was frequently right. Sometimes part would redissolve and part would not, and then the supposition was that it was a mixed fluid, some ovarian and some peritoneal, that an ovarian cyst had burst, and some of the fluid was in the peritoneal cavity; a certain amount of fluid had been poured out, and there was a mixed fluid, which contained some true albumen and some paralbumen.

After a little further examination, one saw that there were

exceptions to this rule. For instance, Dr. Schetelig, of Hamburg, once examined the fluid from a large renal cyst that I had emptied, and he found paralbumen and cholesterine, and no trace of urea, because the proper structure of the kidneys seemed to have been completely destroyed. He found in another patient that the fibrine found at the first tapping of the patient disappeared after another tapping, and he concluded that, at the first tapping, the fluid was removed from the peritoneal cavity, and at the second tapping, from an ovarian cyst. It is quite possible, in tapping the abdomen, to remove a certain amount of peritoneal fluid and leave an ovarian cyst behind; and that, at another tapping, the ovarian cyst might be tapped and emptied, and the fluid escape.

After the chemical examination of the fluids removed, one has to resort to the microscope.

[Speaking of the different kinds of abdominal tumour Mr. Wells observes that solid tumours in the ovary are much more rare than semi-solid ones.]

The different kinds of ovarian tumours and their diagnosis of adhesion may be made out by some little attention in the examination of a patient. We can ascertain with a little care whether a cyst is a single cyst with limpid contents, or whether the contents are likely to be viscid, and this will assist us in forming some opinion as to whether one should be content with tapping and drainage, or whether it would probably be ultimately necessary to resort to ovariotomy. The reasons which would induce us to suppose that the cyst is single, and that it probably may be cured by tapping, are that it may have existed for many years without any great effect upon the general health of the patient; or, on the other hand, that it has formed with such extreme rapidity that it is almost certainly mistaken for ascites. You see a young woman whose abdomen has rapidly enlarged (one may sometimes see this one's self, but it is more frequently reported); but I saw it in the case of a young lady I examined many years ago with Dr. Rigby. Neither Dr. Rigby nor I were at first certain that we could discover anything in the abdomen. We thought we could make out a little dulness and doubtful fluctuation, but there was no enlargement; and yet, in three weeks, that cyst was as large as an adult head. It rapidly increased, and had to be removed in three weeks after the first examination. That is a very rare and exceptional case. When you get this very rapid formation of a cyst, it is pretty certain the cyst is a single one; and so also if the cyst has existed a good many years, and the patient has not suffered much from it, it is probably single. And these are cases in which tapping alone may not only afford temporary relief to the patient, as it does, but may possibly complete the cure; the fluid may never

re-form. In such a case as this, one can detect no hardening of the cyst-wall, no nodules, and the surface is so smooth one can make up one's mind that there are no groups of secondary cysts about it, and that it is practically unilocular. When you get resistance to the wave of fluctuation in different directions, then you may also be pretty certain that there are septa dividing the cyst into different portions, and that it is of a multilocular character, and in that case tapping would be of no use; although one compartment might be emptied, the others would remain full. Supposing we find cartilaginous or hard bony projections, or nodules, in different parts of the cyst, then the probability is strong that the tumour is one of the forms of dermoid tumour, in which we may find cartilage, bone, fat, hair, teeth, and other things, of which we have some good specimens in the museum. Occasionally one begins to doubt whether both ovaries are affected or not. We may meet with a sulcus going immediately down the abdomen, and, if we find intestine there, in that case it is extremely probable both ovaries are affected. Sometimes this is deceptive. I have seen a very large sulcus on the surface of one ovarian tumour, made by the Fallopian tube pressing it in the centre, so that there was a very distinct bulge on either side, and we felt almost certain that both ovaries were affected, whereas only one was diseased. The belief of Boinet, that one can foretell before tapping the character and colour and consistence of the fluid, has a certain amount of foundation. The progress of the disease, the more or less acute pain complained of, the other signs of inflammation more or less rapid, and the state of the general health of the patient, will lead to suspicion probably that one has some purulent fluid, or mixture of blood and serum, or pus in a cyst; if there have been a rise of temperature at night, loss of appetite, and other signs of fever, you may be pretty certain there has been some inflammatory change going on in the substance of the tumour or cyst, which will lead to changes in the condition and character of the fluid.

The solid tumours I need not say anything more about, except that they do not fluctuate; they are solid. These are much rarer than cases where one finds a portion of the tumour solid and a portion of it fluctuating.

Then with regard to the diagnosis of adhesions, it used to be thought a matter of very great consequence to determine, before undertaking to remove an ovarian tumour, whether it was adherent or not; and, in the early days of ovariotomy, great trouble used to be taken by Dr. Frederic Bird and others to ascertain, if they could, whether the tumour was free. They would tap and watch carefully the way the canula moved as the cyst emptied; and Dr. Bird used to put needles through

the abdominal wall into the cyst, to see the way in which the ends of the needles were deflected, and, if there were signs that the cyst was adherent, then it was not considered a case in which ovariotomy ought to be attempted. When I commenced practice in this particular department of surgery, I soon began to doubt whether this was a point of much consequence, and, when one began to number cases by hundreds, there seemed to be very little difference in result between cases of adhesion and of non-adhesion, so that I do not think we need give more time or attention to this point, but it is as well to remind you of it.

I have here some specimens of bone and teeth, from a cyst which I removed, prepared by Dr. Junker, who for five years was in Japan teaching the Japanese surgery, establishing a surgical school there, and carrying the benefits of our surgical knowledge to the Japanese. They illustrate extremely well the sort of bone-like matter one occasionally meets with in dermoid cysts of the ovary.

To revert for a moment to the subject of diagnosis of adhesions. Practically, I think, the question of extent of adhesion to the abdominal wall is not of much consequence. The result is pretty much the same, so far as the adhesions go, to the patient; but the amount and intimacy of pelvic adhesions are a matter of very much greater moment. Supposing an ovarian cyst is adherent low down in the pelvis, between the uterus and rectum or the uterus and bladder, or on either side, the attempt to separate it is necessarily a dangerous one. If any bloodvessels be torn, it is difficult to find them. Although by artificial means one can occasionally throw a strong ray of light to the bottom of the pelvis and secure a bleeding vessel, yet it is troublesome and difficult to do; and the results of these cases are by no means so satisfactory as when there are only adhesions to the abdominal wall. One can pretty well ascertain if there be such adhesions as these, by a careful examination of the pelvis by the vagina. You find that they do not move with the position of the patient; they do not move when she coughs; they do not move when the shoulders are lowered and the hips raised; and it is impossible, either with the uterine sound or in any other way, to separate the uterus from these pelvic portions of the adherent tumours. Occasionally there will be an ovarian tumour low down in the pelvis which does move, from which the uterus can be separated, and need not interfere at all with ovariotomy; and in such cases it is sometimes quite curious to hear the rush of air into the bottom of the pelvis when the lower portion of the cyst is separated from the hollow of the sacrum. Air rushes down with a gurgle, the tumour is easily brought up, and there is no more difficulty than in an ordinary case. It occasionally be-

comes very doubtful whether an ovarian tumour is simply jammed down into the pelvis, or whether it is fixed there by strong adhesion; but still I think this can be made out by care—ascertaining whether the uterus is movable; ascertaining whether the lower portion of the cyst moves in different positions of the patient; and, if it really be firmly fixed, whether the sensation to the finger is that it has been absolutely glued down to the bottom of the pelvis. If there be this intimate adhesion pretty low down, as a rule I think the operation is not one that is likely to succeed.—*Medical Times and Gazette, June* 15 *and* 22, 1878, *pp.* 644, 670.

65.—ON THE SURGICAL TREATMENT OF ABDOMINAL TUMOURS.

By T. Spencer Wells, Esq., F.R.C.S., &c.

In his third lecture Mr. Wells considered the surgical treatment of ovarian cysts and tumours; tapping, by abdominal wall, vagina, or rectum; tapping, with drainage; injection of iodine, or antiseptics; incision and drainage, and ovariotomy; going on to the selection of cases, preparation of patient, instruments, anæsthetics, and duties of assistants and nurse.

With regard to the mode of tapping, the lecturer said, "When tapping through the abdominal wall was performed in the early days, it was done with the patient seated on a chair, an assistant on either side of the patient, a long towel fastened round her, possibly an opening in front of the towel; a pail was placed between the patient's legs, and then the surgeon, taking one of these old-fashioned trocars, with considerable force plunged it into the abdomen. Sometimes a small incision was first made in the skin with a scalpel or lancet, in order to lessen the force necessary to pass the trocar inwards; but it was very soon apparent that the danger which followed tapping in this way was due a good deal, in the first place, to the position of the patient, and in the next to the instrument used. The fainting of the patient, even notwithstanding firm compression by bandages or towels, almost necessarily due to the upright position, could be avoided altogether by having the patient in a recumbent position, not taking her out of bed, simply bringing her to the edge of the bed, lying on her side, and tapping in that position. In that way, no bandages were necessary, and a very large cyst might be emptied without any faintness being felt by the patient. In some cases, the surgeon would tap a patient with a trocar, and it would be what Sir Astley Cooper called "dry tapping"—no fluid followed. That arose from shortness of the cannula in patients having a thick abdominal wall. I suppose this trocar would hardly go through the abdo-

minal wall of a patient having an inch or an inch and a half of fat; by the time it reached the cyst it would barely enter it; and then, when the trocar was withdrawn, the fluid, instead of passing through the cannula, escaped into the peritoneal cavity. Again, supposing one did with a trocar of this kind enter the cyst and removed the trocar, leaving the cannula in the cyst, after a certain amount of fluid had escaped, the cyst contracted and fell off and sank into the peritoneum, so that no more fluid escaped, or it came away only gradually. I have seen all these things occur myself; and the first thing I did in altering the old instrument was to increase the length of the cannula; and by simply lengthening the cannula I avoided both the danger of not opening the cyst, and that of the cyst falling off the cannula. Then Mr. Thompson, of Westerham, made the great improvement, of which there is an exaggerated specimen here. He arranged the trocar in such a manner that when it entered the cyst the trocar was withdrawn beyond the point at which the fluid escaped down the long flexible tube; he thus effectually prevented any danger of air entering from the outside into the interior. He took a good deal of trouble to do this by putting the tube into a vessel of water, sucking at one end until the water rushed up through the tube. By advancing the trocar, he kept the tube full of water, and as the lower end was under water in the pail beside the bed, of course no air could come up and get into the cavity of the cyst when the piston was withdrawn. I soon found that filling the tube with water was quite unnecessary, because it was only a question of the level at which one passed the trocar into the cyst. But there were one or two objections to this instrument. In the first place, there is a little interval of time—a moment—between the entrance of the point into the cyst and the withdrawal of it, in which instant there may very possibly be some escape of fluid between the sides of the cannula and the punctured cyst. I have seen that occur in ovariotomy, and I have no doubt it occurs also in ordinary tapping; so that I was anxious to avoid that moment which is lost between the entrance of the trocar and the removal of the piston leaving the cannula free. It occurred to me that a common steel-pointed pen would do as well as anything; and I thought if that were passed into the cyst, the fluid would run freely away, and it would be easy to make a syphon trocar. After a little trouble, my suggestion was carried out, and the instrument I now show was the result of that trial, and it is the one which I have used ever since. The point is exactly like the point of a steel pen, and is so made that it does not cut a piece out of the abdominal wall, but simply enlarges the opening made by the scalpel. A small opening is made first by a lancet or scalpel, and then this instrument is easily passed into the

cyst. It is evident, if this were left in the cyst, it would be dangerous when the cyst was empty; a sharp point like this in the interior of a contracting cyst would be pretty certain to wound it; so I added a blunt cannula. I first put the cutting point inside, but afterwards found it better to slip up the blunt cannula inside the sharp tube, and that is the form of instrument now used. Passing it with the point directed downwards so as to avoid any rush upwards of air which might occur if the point were at a higher level than the end of the tube. With the tube, the instrument at once becomes a syphon, and this calibre is large enough to allow the escape of viscid fluid. There are several small openings in the cannula, because if the opening be too large, there is the possibility of the entrance of little bits of omentum or small shreds or clots, which may be in the interior of the cyst. These would block the tube; therefore it does not do to make it too large. Fixing a long elastic tube on to the end of this instrument, the fluid is carried silently away into a vessel under the bed; the patient does not hear any splashing, and there is no possibility of any air entering into the cyst. Supposing the fluid—whether ovarian or cystic—is in the peritoneal cavity, or there is a doubt as to where it is, it is desirable to remove such fluid without the possibility of wounding any uterine tumour, or any mass of cancer, or injuring the intestines or omentum. For this operation I contrived this form of cannula, which is used after a simple lancet puncture. Passing a lancet or small scalpel through the abdominal wall until fluid appears, then removing the knife, and pushing this hollow tube to the cavity, the fluid at once rushes away. The tube may be moved about as a probe to feel whether any tumour is free or adhering, and we can do that without the slightest fear of doing any harm. With these several small openings, there is no fear of portions of omentum becoming entangled by the pretty strong suction that goes on through the long syphon.

"Tapping through the abdominal wall in some cases becomes not only a means of temporary relief, but if the cyst be really a single cyst it may be followed by the complete cure or recovery of the patient. I have known, now, a great many cases where, when the cyst proved to be really single or unilocular, it never refilled after the first tapping; the patient was by this very simple means completely cured. So I think we may lay down almost a positive rule, that when we can be sure that the cyst is a single cyst, and we cannot discover any secondary growths in the cyst wall, either by examination by the abdomen or the vagina, we must consider it a duty to see what tapping will do for a patient before thinking of more serious measures. I think I have seen quite enough now to warrant me to endeavour

to impress upon surgeons that if the cyst be a single cyst, before they do anything else they should see what can be gained by one tapping. If the tapping be done with precaution, the risk is extremely small; the patient loses nothing, and may be cured. There are a great many cases recorded in my book on Diseases of the Ovaries to prove that fact, and also to show that the mortality of ovariotomy, supposing it afterwards becomes necessary, is very little affected indeed by previous tappings. I gave a table showing what the effect of one, two, and many more tappings, up to eighteen, had been upon the mortality, and the difference seemed to be very trifling. — *Lancet, June* 29, 1878, *p.* 938.

66.—ON THE TREATMENT OF ADHESIONS AND OF THE PEDICLE IN OVARIOTOMY.

By T. Spencer Wells, Esq., F.R.C.S. &c.

The next point to consider is the mode of dealing with adhesions—supposing the cyst, instead of being free, instead of coming out when it is empty, still adheres, either to the abdominal wall, or some other part of the abdominal cavity, or to the omentum, or intestine, or to some of the structures in the pelvis. It is very possible that, in making the first incision, one has found the cyst is not free, but that it is closely adhering to the abdominal wall in front, and it is necessary to make a separation. Sometimes it may be well to do this while the cyst is tense, passing the fingers between the abdominal wall and the cyst, and, after separating some adhesions, to tap and empty; but if the adhesions be very close, I believe the safest plan is to empty the principal cyst before making any attempt to separate the adhesions. There are some obvious advantages in this mode of procedure. When the cyst is opened, there being only an incision in the abdominal wall, the peritoneum being still undisturbed, of course any fluid that escapes from the cyst cannot pass into the peritoneal cavity. It must run away through the opening made in the abdominal wall, and in this way the fluid can do no harm. Then when the cyst is empty, it is often extremely easy to draw it out by passing one hand into the interior and then grasping the back of the cyst and inverting it, as it were, drawing out the back part through the opening made in the front part. In this way it will often come out without much trouble, whereas if one attempt to proceed to separate an adhering cyst from the abdominal wall, without being very certain where the cyst and peritoneum join, then mistakes are apt to be made. In many cases, when trying to separate closely adhering cysts, surgeons have torn off portions of the peritoneum,

and have got into considerable trouble. When the cyst is closely adhering in front, I believe the best plan is to empty it, to invert it, to draw the posterior part of the cyst forwards, and endeavour to draw it out. If that cannot be done, and there are still firm adhesions, then it is necessary to proceed systematically, and find out, by carrying the incision upwards and downwards, exactly where the limit is between the cyst and abdominal wall; and proceeding to that point, one hand draws the cyst outwards, while with the other hand we separate it. Occasionally, some very firm band must be divided with the knife or scissors, but it is very rarely this is the case; it almost always can be done without dissection. Then when the adhesions have been separated, it may be occasionally necessary to break up the tumour—if it is too large to come out through an incision of moderate length—and this may be done either by tapping any distinct cysts, or with the fingers tearing the septa which divide the cavity into several compartments; and when the contents of each compartment are pressed out, the main cyst usually follows. If there be a great deal of solid matter, it may be necessary to extend the incision till the opening is sufficiently large to allow the tumour to pass outwards without using too much force.

The tumour having been drawn out of the abdomen, the pedicle comes into view. It is seen that the cyst is connected with one or other side of the uterus by a band of greater or less breadth, or by a sort of cord or root, long or short. This cord or pedicle may possibly be divided into more than one portion, but at any rate the connexion between the uterus and the cyst is seen or felt, and this must be secured before it is divided. There are various ways of dealing with the pedicle. In the earliest operations, the pedicle was tied with whipcord. and the ends were brought out through the wound. After a time, leather ligatures were used and the ends were cut off short, and the ligature, with the pedicle, was returned into the abdominal cavity, and allowed to sink down into the pelvis. Still later, the use of the clamp was introduced by Mr. Hutchinson, and was followed by me and by others.

The different modes of using ligatures, the clamp, and the cautery, I will presently describe; but I will first show the clamp which, after a good many trials, is the one that is now used. This is a cyst I removed last week. Here are the remains of the pedicle and the Fallopian tube; and this is where the cyst was separated from the side of the uterus. Now, suppose that the cyst has been drawn out through the opening in the abdominal wall, and that round the neck or pedicle between the cyst and the uterus, the clamp is applied in this manner. It is passed round at the precise spot selected, and is firmly grasped

by these handles. Then the surgeon holds the whole perfectly secure while he fastens the screw on the arc. I generally prefer, instead of trusting to my fingers, to take a pair of forceps, and, giving an additional turn to the screw, to fix the clamp so that there is a certainty that it will not slip. Occasionally, when one part of the pedicle is very much thinner than another part, it is possible the thin part might slip through and troublesome bleeding might follow. If there be any fear of that, it is a good plan to pass a ligature round the pedicle below the clamp and tie it. If the silk be passed round before the clamp is thoroughly tightened, it makes a circular constriction, presses it altogether into a circular form, and then the clamp is fastened. There are several advantages in doing this. You see exactly where you are going to put the clamp; it scarcely takes a moment of time, and putting the clamp over the tied part, it is so tightly held that there is no possibility of any thin part slipping out. That is the way in which the clamp is now used. There is the clamp. It is curved; whatever tension there may be, it does not press upon the sides of the wound. It holds the pedicle very firmly against the abdominal wall, and keeps any dead or dying portion of the pedicle well outside. The part that is left above the clamp outside the abdominal wall is very small, and upon this one sprinkles a little perchloride or persulphate of iron. This converts what would otherwise become putrid almost into hard leather, which cannot do any possible harm.

Supposing the pedicle is too short or too broad for the clamp to be used, or the surgeon wishes to use the ligature in any particular case, then it is easily done by one of these forms of needle. They are all made with blunt points. It is dangerous to use sharp-pointed instruments near the abdominal cavity, or with portions of small intestine occasionally coming in the way; and it is also advisable to avoid any wounds of the large veins which are frequently met with in a pedicle. Then, to avoid troublesome entanglement of the silk, I have had a double eye made, so that, after the instrument is past the ligature, it is left almost ready for use. Even through the thick part of the wall of this cyst it is not difficult to push the blunt point, and then, when the instrument is withdrawn, two threads are ready to be tied. I first make what is known as the "surgeon's knot," so that there is no probability of its slipping, and I like an additional knot also, because I believe there is a greater safety in the surgeon's knot with a third knot than in any reef-knot. With the third knot and the pedicle tied in two or more parts, I believe it is, as a rule, secure; but sometimes, just at the point where the needle has been passed, the pedicle may split in a very troublesome way. The thin membrane of the broad ligament seems to separate, or the opening made by the needle extends

or splits downwards, and blood oozes from it. If so, all that is necessary is to take another turn of the silk and just bring it round from one side to the other, so that the whole pedicle is compressed. The objection to doing that is, that it keeps the broad ligament rather too much on the stretch. It is better to leave it, if one can, with the different portions tied separately, than to tie it altogether. It crumples up the whole of the surface, and makes a tight band across the pelvis, which it is desirable to avoid.

Be sure that every sponge passed into the peritoneal cavity is removed. There are cases on record in which a sponge has been found in the peritoneal cavity of a patient after her death. Once it occurred to me to make this mistake myself; but I got the sponge out afterwards.

Now a word or two about the cautery treatment. It originated in this way: Mr. John Clay, of Birmingham, contrived this instrument for separating adhesions with the cautery. This is what is called "Clay's adhesion-clamp." He passed it over any firm adhesion or any adhering portion of omentum, screwed it up, and then, with a cauterising iron heated in the fire, he burnt it off. There is a little rim here which prevents the cautery from slipping. Very soon after Mr. Clay had introduced this plan for treating adhesions and adhering omentum, Mr. Baker Brown extended the plan to the treatment of the pedicle. He found it did so well for adhesions and omentum that he thought it would also do well for the pedicle; and here is one of his original instruments. The instrument is in two pieces. It is fastened on to the pedicle with a screw. When fastened, one of the irons, heated in the fire, was gradually passed along the cyst on the upper surface of the clamp. A guard prevented the iron slipping, and an ivory plate prevented the clamp getting overheated and burning the patient. Of course, with a large tumour, that was sometimes a troublesome process, and after a little practice I began to cut off the cyst; and then, with an ordinary druggist's plaster-spreading spatula, well heated, and used slowly, so as to burn or scorch the portion of the pedicle between the two blades of the clamp, it was very seldom in opening it that the vessels bled; but every now and then one or more did bleed. Various contrivances were made to avoid damage from that. The most convenient was the Russian, or Maslowsky's. The pedicle was seized with that; and then, as it only holds the cauterised part in certain portions, it was seen whether it was bleeding or not. Supposing it did bleed, then any bleeding vessel was tied. In a good many cases where the cautery was used, some vessels did bleed, and we had to resort to the ligature; and it was this uncertainty which led me, after great success with its use—I think out of sixteen cases

in which I tried it, only two died—finding it uncertain and troublesome, and that we were obliged often to use the ligature instead of trusting to the cautery only, I gradually abandoned it, and, in cases where I could not use the clamp, resorted to the ligature only. There are various forms of cautery and clamp. This is Maslowsky's cautery-clamp, which is a very convenient instrument, and has the advantage of two little catches which enable one, in opening it, to see whether the charred tissue is bleeding. Here is a piece of talc, the best non-conductor of heat known, which is placed beneath the clamp to guard the skin from the effects of the hot iron. And here is a very ingenious clamp made by a late colleague of mine, Dr. Wright, who had seen me troubled in one or two cases by bleeding in opening a cautery-clamp; and he arranged a set of screws by which one could open portions of the cautery in succession, to see whether any vessels bled or not. Once or twice I found it had the disadvantage, in opening it, of separating the two layers of charred tissue, and in that way setting up bleeding, which otherwise might not have occurred.

Different forms of clamp for use without any cautery, where we trust to the clamp alone, are here on the table. Mr. Hutchinson was the first to introduce it, and he first used the common "calliper" of the carpenters. This is simply a compass fixed by an arc and screw. That is the instrument which Mr. Hutchinson used in his first cases, and it is really a very effective instrument. The pedicle is very well compressed by the calliper, and is quite tightly held. His first improvement was to make the handles so that they could be taken off, leaving the clamp only lying upon the abdomen of the patient, without any handle. Then there were trials made to make the blades act in a more parallel way. Here is one which I have kept, as showing the amount of force I occasionally put upon the pedicle. This bend I actually made upon the pedicle of a living patient. When I took the clamp off, this strong piece of steel was bent in the way you see. Then I tried to get a clamp like the hemorrhoidal clamp, as it is called. If I could get that action upon a pedicle, I thought we should get circular compression without the necessity of applying a ligature. Here is Kœberlé's clamp, which he still uses, although it is extremely sharp, and evidently only intended for a small pedicle. After it is secured, the two rings serve to keep it from being drawn into the abdomen. The nearest approach to the circular clamp I succeeded in obtaining was made for me by Mayer and Meltzer. It acts so as to produce a very complete circular compression. It is easily fastened by the screw. But I found this (although it appears smooth and blunt enough) more than once cut the pedicle behind the place where it was applied, and led to troublesome and dangerous

bleeding, so that I was obliged to abandon that; and, although I see it frequently figured in books, particularly foreign books, as my clamp, it is a clamp I have given up for the last seven or eight years.

Supposing one wishes to use the cautery, this is the well-known instrument of Paquelin, called a "thermocautère," and if there should be a return to the use of the cautery, in ovariotomy, this would be the most convenient instrument one could use. It is simply a hollow platinum cautery, first heated in a spirit-lamp, and then, by sending a stream of benzoline through the platinum end, a dull red heat or white heat can be kept up and maintained as long as the surgeon wishes it. Irons taken from a fire are clumsy. Copper cauteries are less liable to rust and scale off than iron; but platinum has the very great advantage of not rusting, and also the advantage that an assistant can, by working these bellows, keep up any degree of heat required, and the surgeon can use his cautery as long as he pleases. There is one little imperfection in the instrument: there should be some fastening to secure the plug in the bottle. If one use the bellows a little too fast, the air accumulates in the bottle rather faster than it passes through the tube, the plug flies out of the bottle, and the cautery cools just at the time it is required.—*Medical Times and Gazette, July* 6, 1878, *p.* 19.

67.—ON SOME POINTS IN OVARIOTOMY.

By T. Spencer Wells, Esq., F.R.C.S., &c.

With regard to any supposed advantage of wire for ligature, I may say that I have never used it in women, because some experiments which were made for me by Professor Gamgee at the Veterinary College upon sheep seemed to prove that there was no advantage in the wire over silk, whereas it did set up a good deal more irritation in the parts surrounding it than silk did. The relative advantages of the clamp and ligature may possibly be best learned from the results of actual work. There is no doubt a very great advantage in the clamp at the time of performing the operation. It is put on quickly, and bleeding is suppressed with complete security. In all the cases I have done, I have never known a single instance of dangerous bleeding in consequence of the slipping of the clamp; whereas after the use of the ligature I have known that occur. I myself have lost two patients from primary hemorrhage a few hours after the operation, simply from the slipping away of the broad ligament from the grip of the ligature, internal bleeding taking place, and the patient actually dying of primary hemorrhage. But after the use of a good clamp, I have never had a case of serious bleeding. I know also that this accident has happened

to other surgeons of my acquaintance, and has proved fatal; therefore in estimating the advantages and disadvantages of the two methods, one must allow that, so far as regards security from primary hemorrhage, the advantage is altogether on the side of the clamp. The advantage of the ligature is that it allows the wound to heal with less interference, less frequent dressing is necessary, and the patient is certainly, in the first few days after the operation, in a more comfortable state than occasionally happens after the clamp. Very often the patient is quite unaware of a clamp being there. It does not cause the slightest inconvenience. But occasionally there is a certain amount of unpleasant discharge around it, and some redness and swelling where the lips of the wound are brought round the pedicle, which one does not meet with in the wound when the ligature is used. In this respect, the balance of advantage is in favour of the ligature; but the main question is, of course, what is the proportion of recoveries? I will not attempt to calculate what this may be in the hands of other surgeons, but I can compare the results of the clamp and ligature in my own practice. I have here a table of the results of 900 cases. I completed the 900th operation last week; and I have gone into the result of the treatment by the clamp and by the complete intra-peritoneal ligature, or what we call the ligature returned. I find there were 627 cases treated with the clamp, of which 130 died; that is a mortality of 20·73. With the ligature 157 cases were treated, with sixty deaths, or a mortality of 38·2. Speaking simply, the mortality is nearly doubled; the mortality of the same operation in the hands of the same operator is 18 per cent. more with the ligature than with the clamp.

"If the pedicle be small enough to be securely held in a clamp of moderate size, and long enough to permit the clamp being fixed outside the closed wound, without much pull on the uterus or broad ligament, no more ready or more successful method than the clamp can be desired. When the pedicle is so short, broad, or thick that a very large clamp would be necessary to secure it, that the wound could not be closed around it, and the traction upon the uterus when the clamp was fixed outside the abdominal wall excessive, either the ligature or the cautery should be preferred to the clamp. Or if, after applying a clamp, it be found that it cannot be fixed outside without undue strain upon the uterus, either the cautery may be used, or the pedicle may be transfixed and tied, and the clamp removed after it has served its temporary purpose." To that I have now very little to add.

With regard to the treatment of torn adhesions, supposing they go on bleeding, which they do not very often if they are only torn, I should say by tearing them one adopts the

most certain means of preventing bleeding. As a rule, if adhesions be torn and the tumour removed, the abdominal wall contracts, and this contraction stops the bleeding; but if the adhesions be cut by scissors or knife, they do sometimes bleed, and in a troublesome way. There are two or three different modes of dealing with them. The torsion forceps may be left on for a few minutes, and very often their compression will be sufficient to stop the bleeding. Occasionally it may be necessary to put a ligature on some bleeding point. I have met with very troublesome bleeding from a large mass of varicose veins on the inside of an abdominal wall. In that case, I passed two needles from within outwards, and tied a silk suture over a pad placed outside. Professor Simpson passed a thick needle in one or two cases through the abdominal wall, and stopped bleeding by the acupressure. But in the only two cases in which I have myself seen much trouble from oozing of blood from separated adhesions, I managed it in a very simple way. There was plenty of lax abdominal wall to deal with, so I simply held the two loose folds of the abdominal wall firmly together while the sutures were being introduced, sponged out the cavity, kept up the pressure, and doubled or folded the two bleeding surfaces against each other, and doing as Dr. Kimball of Lowell had done before me, though without my knowledge, I passed a couple of needles through the whole thickness of the abdominal wall. Suppose the inner surface of the abdominal wall is bleeding, the two sides are pressed together, and a long strong needle is passed above and another below, and then by a pad on either side—I used cork—the two surfaces are pressed together. Very firm pressure is made of the two oozing surfaces one against the other. In my case there was no bleeding whatever; and after two days, when the lax abdominal wall had contracted, and the needles were removed, it really would have been quite impossible to discover that anything unusual had been done.

We have succeeded distinctly in lowering temperature, and in keeping it low, by the application of ice or iced-water to the head. The first trials were made after a suggestion of Dr. Richardson, by putting an ice-bag round the neck. Dr. Richardson believed that, by icing blood that went through the carotids to the brain, and blood that came back through the jugulars, we should directly lower the temperature of the brain itself; and probably it may have been done experimentally, but in practice it was not found easy to do. It was difficult to keep any sort of cravat or collar that we tried filled with ice round the neck of the patient; it slipped off, and so we were reduced to the old india-rubber bag, or the ice-helmet, which is so well known in lunatic asylums. But that was inefficient; it did

some good, but it was not enough, and, after a time, Mr. Thornton contrived this form of cap, which answers the purpose extremely well. A pail of water, with a large lump of ice in it, is placed by the bedside of the patient, and the stream of iced-water runs through the cap, which is formed of a series of tubes of indiarubber, lined with linen. This is placed upon the patient's head. It is made of different sizes to fit different shaped and sized heads; and this other tube is put into a second pail on the other side of the bed, and by this means the head is iced. The effect in lowering temperature is very marked. It is very seldom that within an hour the effect upon the patient cannot be distinctly proved by the thermometer, and I believe in many cases it has been of very signal service. If the temperature be rising, the rise is checked; and if it be very high it can be lowered. A great many of the evil results of ovariotomy seem to be due to the fact that the temperature of the body for a time is too high. The brain receiving blood of a temperature five or six degrees higher than it has been accustomed to, does not give its orders to the secreting organs as it should do, and they all suffer in consequence. The kidneys do not act; there is no action of the bowels; all the processes of nutrition and secretion suffer, because the brain is overheated. If we can keep the brain cool, all the functions of the body go on much better, and we gain time for the recovery of the patient.—*Medical Times and Gazette, July* 13, 1878, *p.* 42.

ADDENDA.

68.—ON A NEW TEST FOR CHLORAL.

By Dr. Francis Ogston, Jun.

The addition of stale sulphide of ammonium to a solution of chloral hydrate of moderate strength, say 10 gr. to the drachm (the strength of the British syrupus chloralis hydratis), causes, in a short time (*not immediately*), the colourless liquid to assume a slight orange-yellow colour, the liquid remaining clear; on letting it stand, the colour gradually deepens to a brown, and now a cloudiness comes over it, which in about half an hour, or perhaps longer, is deposited as an amorphous precipitate of a brown colour, and which appears to consist of sulphur.

While the change of colour is going on and the brownness appears in place of the orange, a gas is given off of a most offensive smell, apparently a mixture of chloroform and hydrogen sulphide, with something in addition, in such quantity as to fill a large room in a few minutes.

In order to find out the smallest quantity of chloral hydrate which would give a reaction with ammonium sulphide,

(*a*) I dissolved 1 gramme of it in 100 cc. of water, took 1 cc. of the solution and added 1 cc. of ammonium sulphide, when the solution became of a canary yellow colour, passing through orange-yellow to brown in the course of about six hours, letting fall a slight brown precipitate and giving off a characteristic smell.

(*b*) I then took 10 cc. of the above 1 p.c. solution, which I put into 100 cc. of water, and, on adding 1 cc. of ammonium sulphide to 1 cc. of this solution, I got in about half an hour a slight straw colour which had deepened to a pale orange colour in twelve hours, giving off no smell.

(*c*) Lastly, I took 1 cc. of the solution *a*, which I added to 100 cc. of water. Of this solution I took 1 cc., and added 1 cc. of ammonium sulphide, and, after standing for eighteen hours, the liquid had changed to an exceedingly faint straw colour.

Thus 1 cc. of *a* solution (0·01 gramme of chloral hydrate) gave the brown colour, the precipitate, and the smell.

1 cc. of the *b* solution (0·001 gramme) gave an orange colour, but neither precipitate nor smell.

1 cc. of the *c* solution (0·0001 gramme) gave a very faint straw colour.

The first and second of these results are, I think, sufficient to

establish the presence of chloral hydrate; the third I should hardly trust to, as the reaction was so faint, though I verified it by repetition of the experiment.

I may further state, that the similarity of the reaction of ammonium sulphide with chloral hydrate to that with antimony salts, suggests a caution in accepting the orange coloration alone as a certain test for either poison. Antimony, on the one hand, may be known by the orange precipitate thrown down by the addition of an acid, which does not follow when it is chloral hydrate which is present; and chloral hydrate, on the other, by the fact that the orange colour deepens on standing to a dull brown.

In conclusion, chloral hydrate alone seems to give this reaction with ammonium sulphide, for I have submitted many of the substances of similar chemical construction to the test without any such result following. Among those were chloroform, chloric ether, for micacid, &c.—*Edinburgh Med. Journal, Oct.* 1878, *p.* 292.

69.—ON BUTYL-CHLORAL.

By Professor Oscar Liebreich. (Reported by Dr. Graham Brown.)

In the course of a lecture which he delivered on the 18th of December, 1877, and which I had the advantage of hearing, Professor Oscar Liebreich, in treating of butyl chloral, made some observations which may be of use to English medical men.

In referring to the chemical composition of this substance, the learned lecturer pointed out that the name which was formerly applied to it—croton chloral—was incorrect and very misleading, as it has no relation to croton oil, and that it ought therefore to be discontinued.

In the human subject, as well as in the lower animals, the first action of the drug is to produce anæsthesia, which begins in the head and gradually passes to the rest of the body, reflex irritability remaining intact in the limbs for some time after it has been abolished in the head. Then follows narcosis; but it is important to observe that anæsthesia in the head and face may be complete before any trace of narcosis manifests itself. From a consideration of this fact Professor Liebreich suggests the use of butyl chloral in operations on the face, in cases where other anæsthetics are contra-indicated. The dose for this purpose is from one to two grammes (15 to 30 grains).

The chief interest of the lecture lay, however, in the remarks made as to the action of butyl chloral on the heart, and the

manner of death in fatal cases. It was pointed out that whereas in the case of chloral hydrate the heart was paralysed and its action ceased before the movements of respiration came to an end, the reverse held good in the case of butyl chloral. A fatal dose of that substance attacks the respiratory centre in the medulla oblongata and paralyses it, while the heart is unaffected and continues to pulsate for some time after death.

To demonstrate this fact Professor Liebreich showed two rabbits, both of which were in a state of complete narcosis, the one from the effects of chloral hydrate, the other from butyl chloral. In the case of the first animal the thorax was opened just as all respiratory movements ceased. The heart was lying motionless, both ventricles being dilated and filled with blood. Artificial respiration had no effect in reproducing the cardiac contractions. But, on the other hand, when the thoracic cavity of the rabbit poisoned with butyl chloral was opened (at exactly the same stage) the heart was found to be beating regularly, and to all appearance normally. After about two minutes the beats, which during that period had become slow, ceased. Artificial respiration at once restored the pulsations, and so long as the rhythmical inflation of the lungs was kept up, the contraction of the heart continued; thus affording an admirable illustration of the views as to the action of the drug which had been enunciated in the course of the lecture. Professor Liebreich also indicated that this property gave the drug considerable advantage as an anæsthetic for vivisectional purposes, especially in England.

The lecturer, in conclusion, adverted to the value of butyl chloral in cases of tic douloureux, and advised its administration, dissolved in glycerine and water, rather than in alcohol, in doses of from one to two grammes.—*Lancet, July* 26, 1878, *p.* 82.

70.—A CASE OF CARBONIC ACID POISONING TREATED BY INHALATION OF OXYGEN:

WITH A DESCRIPTION OF A NEW APPARATUS FOR RENDERING IMPURE AIR RESPIRABLE.

By Dr. CHARLES B. BALL, Surgeon to the Blaenavon Iron Works, Pontypool.

On January 16th, 1877, I attended a family who were found insensible in bed that morning. I found a man aged 55, his wife aged 48, and daughter aged 16, in one bed, in a very small room, quite insensible. There were the remains of a large fire in the grate, the chimney of which was imperfect. Upon admitting plenty of air and using the usual restoratives, the woman became sufficiently conscious to say that she remembered being up at 2 a.m., when she experienced considerable difficulty

in breathing and a severe headache; after which she remembered nothing further till she was restored to consciousness. The man became semiconscious, but the girl remained perfectly insensible. I had her, therefore, removed to another and well-ventilated room, where artificial respiration was resorted to. She now had violent tetanoid convulsions, at intervals of a few minutes. The muscles of the face were, however, exempt from the general spasm. The pupils of the eyes were of average diameter, but dilated in bright light and contracted when the light was removed. This phenomenon was also noticed in the man's case.

During the day, the man and woman progressed favourably, but the girl became much worse, the convulsions appearing at longer intervals and the intervening coma being more profound. The treatment now adopted in her case was the application of a blister to the back of the neck, stimulating enemata, and the hypodermic injection of pure ether. None of these measures had the desired effect, and at ten o'clock that night she was apparently dying. The convulsions had entirely subsided. The pulse at the wrist could not be felt, and the respirations were few and shallow. Through the kindness of Mr. P. C. Gilchrist, F.C.S., I now obtained a large supply of pure oxygen, which, by a little arrangement of an ether-inhaler, I was enabled to give her to breathe in any degree of dilution required. At first equal parts of air and oxygen were given, but afterwards the proportion of oxygen was increased. It produced a rapid and most marked effect; the pulse soon became perceptible, and quickly increased in force. After the inhalation of about four gallons of gas, it was discontinued, respiration and circulation having been well revived. During the inhalation, the convulsions recurred, having been absent for several hours. Although not sensible during the night, she was decidedly better, being able to swallow fluid nourishment, and the convulsions gradually subsiding. She did not recover consciousness till the morning of the 18th, being forty-eight hours from the time I first saw her.

The different degrees in which the persons suffered, although exposed to the same poison, appears to be worthy of note. The intensity of the girl's symptoms is, I think, accounted for by the fact that she suffers from phthisis, and has therefore a smaller available lung-surface. As the woman got up to walk about the room during the night, she for the time respired a higher, and therefore purer, atmosphere; and this may account for the fact that her symptoms were the lightest of the three. The action of the pupils with regard to light, noticed in two of the cases, was remarkable and very apparent. Although the inhalation of oxygen has been recognised in medicine since the

time of Sir H. Davy, yet its use amongst the generality of practitioners is not frequent, and the above case was the first time I have ever used it. Its action impressed me greatly; for I have never witnessed a life so certainly saved by any other therapeutic agent. One great objection to its more general use has been the difficulty of obtaining the gas readily, in a state of purity and in a portable form. This has now been obviated; and I have here a copper flask, into which fifteen gallons of pure oxygen have been compressed. I obtained it from Messrs. Barth of 26, Duke Street, Bloomsbury, and it appears to be very convenient for medical purposes, being easily carried about, as its weight is only five pounds three ounces when full; the diminution in weight will tell roughly the amount of gas used.

From my experience in this case, I was led to the construction of an apparatus for obviating the bad effects produced by inhaling an atmosphere containing a large percentage of carbonic acid, by the simultaneous inhalation of an increased supply of oxygen; the applicability of some such apparatus being demonstrated by the experiments of Regnault and Reiset,

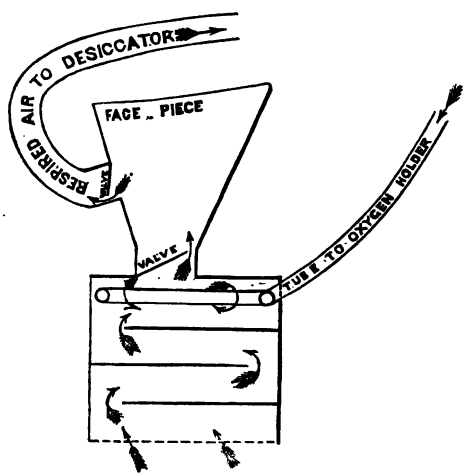


Fig. 1.

who found that animals could live in an atmosphere containing a large quantity of carbonic acid, provided the percentage of oxygen was proportionally increased. The action of this apparatus is partially illustrated by the annexed diagrams. (Fig. 1.) It consists essentially of a tightly fitting face-piece,

which communicates with a chamber of a capacity sufficient for one inspiration. The chamber is separated from the outer atmosphere by a sheet of wire gauze, and the air is made to traverse several partitions. Behind the partition next the face-piece, oxygen is allowed to escape from a perforated tube, which is in communication with a reservoir, carried in the manner of a knapsack. The tube is supplied with a tap, which will allow the escape of oxygen up to 30 per cent. of the air respired. During the intervals of respiration, the diaphragms will prevent the escape of oxygen from the chamber; and at each inspiration, the amount of oxygen required will be washed through the valve at the face-piece by the external atmosphere. The expired air is made by a system of valves to pass through an elastic desiccator, and thence supply a lamp. (Fig. 2.) By this means, the lamp can be made a guide as to the amount of oxygen required. By the use of this apparatus, I expect that a collier would be enabled to go into a mine charged with carbonic acid. In the case of an explosion, in which all the oxygen had been converted into carbonic acid and steam, the explorer would be enabled to supply himself with such an amount of oxygen as would neutralise the effects of the carbonic acid. The man could easily carry a supply of oxygen which would last for two hours, even when he was using the maximum quantity required; and, by connecting a second tube and face-piece to the oxygen-reservoir, he could revive any sufferer whom he might find overcome by carbonic acid. I may mention that, in the experiments connected with the construction of this apparatus, I have myself respired, without feeling any discomfort, during a considerable time, a mixture containing 18 per cent. of carbonic acid, but in which the oxygen was increased to 30 per cent. This amount of carbonic acid is 6 per cent. more than that found after colliery explosions in which complete combustion has taken place.—*Brit. Med. Jour., April* 20, 1878, *p.* 562.

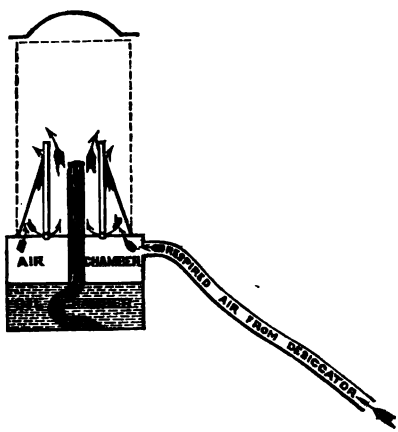


Fig. 2.

71.—ON MORBID CRAVING FOR MORPHIA.

By Dr. EDWARD LEVINSTEIN, Medical Director of the Maison de Santé, Schöreberg-Berlin.

[The following is a review of a Monograph founded on personal observation. The subject is one of much interest.]

The scope of the work, as the author tells us in his preface, is limited, its object being to clearly demonstrate the ill effects which continued injections of morphia have upon the human body (we may add, and mind); to show the dangers threatening society by such a continuous use of the drug; and to point out the remedies for the redress of this abuse. Both the author and his translator have done their parts well, and have made a valuable contribution at once to therapeutic knowledge and to social welfare. We were hardly prepared for the information that the use of morphia by subcutaneous injection was carried to such an alarming extent in Germany,—we hope things are different in our own country—not by authorized practitioners, but by patients—not for the simple allaying of pain, but to lull mental worries and anxieties, and to gratify a sensual craving. Of course, in most cases, the habit originates in the successful relief of physical pain; still, there is reason to fear, that not a few victims have been led to adopt it without so good a reason, and have gradually—in some cases sooner, in others later—become its abject slaves. The doses administered in this way seem almost incredible, both as to size and frequency. Six, eight, ten, twelve, sixteen grains are mentioned as no extraordinary allowance daily; and one case is narrated in which the patient, an officer in the army, injected himself thirty times in the twenty-four hours. The question natually arises: Would it not be easier for such a patient to swallow a corresponding quantity of the drug than to subject himself to so many operations, in themselves not without pain, and when multiplied, likely to cause local irritation. Various reasons for preferring the injection may occur. Probably the true one is that the habit commenced with the treatment of a localized pain, and so was not unnaturally persevered in. Fashion, springing out of popular talk, may also be credited in some measure with the result.

The symptoms of chronic-morphia poisoning are depicted forcibly. The evil consequences arising from the abuse of injections generally begin to appear after a period of four to six months; but there are cases in which these are deferred for years. "Many people afflicted with morbid craving for morphia feel quite well for a certain time whilst using the narcotic; no disturbances are felt, the appetite, and even the bodily weight remains unaltered, while others become emaciated.

Soon, however, a series of morbid appearances show themselves, originating in the cerebro-spinal and sympathetic nervous system. The different organs are affected in various ways: some are not implicated at all; in others, one distinct symptom predominates over the rest to such an extent as to be the chief cause of the patient's complaints." "The eyes are often devoid of lustre, the patient's glance is weak, miserable, and shy; after administering a new injection, however, they become animated, passionate, or enthusiastic. Double vision and a diminished power of accommodation are not rarely met with. The pupils are generally contracted, frequently disproportionate, rarely enlarged." Other symptoms affecting the stomach, the circulation, the kidneys, the organs of generation, the central nervous system, are described at considerable length.

The symptoms caused by abstinence from morphia are very dreadful and may be quoted. "Only a few hours have passed since using the last injection of morphia, and already the feeling of comfort brought on by the action of the drug is passing off. The patient is overcome by a feeling of uneasiness and listlessness; the feeling of self-consciousness and self-possession is gone, and is replaced by extreme despondency; a slight cough gradually brings on dyspnœa, which is increased by want of sleep and by hallucinations. The vaso-motor system shows its weakness by abundant perspiration, by the dark colour of the face, which replaces the pale condition apparent during the first few days. Flow of blood to the head and palpitation of the heart with a hard pulse soon show themselves. The latter symptom often disappears suddenly, and is replaced by a slow, irregular, thread-like pulse, which is the sign of the beginning of a severe collapse. The reflex irritability increases; the patients begin to sneeze, and have paroxysms of yawning; they start if any one approaches them; touching their skin causes cramping movements or convulsions; the trembling of the hands, if not already evident, now becomes distinctly perceptible. The power of speech is disordered; lisping and stammering take place. Diplopia and disorders of the power of accommodation, frequently accompanied by increased secretion of the lachrymal glands, show themselves. The patients are overcome by a feeling of weakness and total want of energy, and are thus compelled to lie in bed. Neuralgic affections of various parts of the body, pain in the front and back of the head, cardialgia, abnormal sensations in the legs, associated with salivation, coryza, nausea, vomiting and diarrhœa, tend to bring them into a desperate condition. Some persons will bear up with fortitude under all these trials; they will quietly remain in bed and endure the unavoidable suffering, hardly uttering a complaint. Of the others, although the great

minority of them sleep and dose during this trying time, some can find rest nowhere; they jump out of bed, run about the room in a state of fear, crying and shrieking. Gradually they become calmer, although, occasionally, their excitement increases. A state of frenzy, brought on by hallucinations and illusions of all the senses, at last causes a morbid condition to which I have given the name of *delirium tremens*, resulting from morbid craving for morphia, it being similar to that caused by alcohol. Some of the patients, however, will be found walking about in deep despair, hoping to find an opportunity of freeing themselves for ever from their wretched condition." A number of graphic cases are given, to which we refer our readers.

It is noteworthy that the symptoms brought on by the abuse of morphia coincide strikingly with the pathological conditions for which the drug is therapeutically administered. Sleeplessness, hyperæsthesia, neuralgic complaints, mental anxiety, depression and excitement are all combated as well as created by morphia. Let it be noted, also, that the principal symptoms showing themselves in people suffering from morbid craving for morphia, during the period of abstinence, are soon removed, for the time being, by the use of morphia.

We may mention here that numerous experiments were performed by Dr. Levinstein on pigeons, on pregnant rabbits, on pregnant dogs, on rabbits and dogs not pregnant, with the view of illustrating the general effects of morphia on the animal system, and its special action on the generative and urinary organs. It appears to us that much unnecessary suffering on the animals thus subjected to experiment was inflicted without any corresponding advantage, without any reliable addition to our knowledge.

We have already mentioned that among other deleterious consequences of excessive use of morphia, impotence in the male, and amenorrhœa and sterility, or proneness to miscarry, in the female, are generally met with. The urine is usually albuminous, and, in fatal cases, the presence of sugar indicates an affection of the *medulla oblongata*.

In the treatment of cases in which a morbid craving for morphia has become incontrollable, Levinstein, whose experience has been extensive, gives a decided preference to the sudden and complete deprivation of the drug to a slow and gradual deprivation. We think that he makes out his case; but our space forbids us to enlarge.

As every one must see, great firmness and courage are required in the physician who undertakes such formidable cases. Sometimes there is real danger from *collapse*; although to ascertain its presence demands much attention and discrimination, as it may be simulated. When it threatens, Dr. Levinstein

finds that a small quantity (half a grain) of morphia injected will usually avert the danger. If, in ten minutes, the pulse and general condition do not improve, the injection may be repeated once or twice, until the return of the normal condition is evident. "At the same time the patient is to be kept awake by counter-irritation, smelling salts, cold compression on the head, dropping ether on the skull, by talking loud to him and shaking him; and stimulants, such as liquor ammoniæ, champagne, port-wine, brandy, hot coffee or tea with rum, are to be given internally."

"It is worth noticing that injections of morphia administered during the collapse do not prolong the time of the severe symptoms of abstinence."

A process is given—that of Professor Dragendorff—by which morphia is readily detected in the urine. This is important with reference to the following observations: "In cases of morbid craving for morphia, the evacuation is not very rapid, as the urine contains morphia for six or eight days after the commencement of the abstinence. A patient, whose urine contains morphia for a longer period than six or eight days, *is sure to be continuing the injections of morphia, although he may positively deny the fact.*" The same sad demoralization which is met with in drunkards exists in the slaves of morphia.

Although subcutaneous injection, like every other benefit, is liable to abuse, no unprejudiced mind will deny that the invention of our distinguished townsman, Dr. Alexander Wood, was a very valuable addition to our therapeutic resources, and, in truth, will mark an epoch in the modern practice of medicine and surgery. We have long felt that, like other inventors deserving of cordial thanks from their fellow-men, his undisputed claims to the original idea of subcutaneous injection, and his perfect application of it, have been inadequately recognised by his contemporaries.—*Edinburgh Medical Journal, August*, 1878, *p.* 160.

72.—RULES FOR THE DIRECT METHOD OF ARTIFICIAL RESPIRATION IN ASPHYXIA FROM DROWNING.

By Dr. BENJ. HOWARD, A.M., M.R.C.S., Eng., formerly Lecturer on Operative Surgery in the Med. Dept., University of New York.

RULE I.—*For Ejection and Drainage of Fluids, &c., from the Stomach and Lungs.*

Position of Patient.—Face downwards; a hard roll of clothing beneath the epigastrium, making that the highest point, the mouth the lowest. Forehead resting upon fore-arm or wrist, keeping mouth from the ground.

Position and action of Operator.—Place the left hand well spread upon base of thorax to left of spine, the right hand upon the spine a little below the left, and over lower part of stomach. Throw upon them with a forward motion all the weight and force the age and sex of patient will justify, ending this pressure of two or three seconds with a sharp push, which helps to jerk you back to the upright position. Repeat this two or three times, according to period of submersion and other indications.

RULE II.—*To perform Artificial Respiration.*

Position of Patient.—Face upwards; the hard roll of clothing beneath thorax, with shoulders slightly declining over it. Head and neck bent back to the utmost. Hands on top of head. (One twist of handkerchief around the crossed wrists will keep them there.) Rip or strip clothing from waist and neck.

Position of Operator.—Kneel astride patient's hips; place your hands upon his chest, so that the ball of each thumb and little finger rests upon the inner margin of the free border of the costal cartilages, the tip of each thumb near or upon the xiphoid cartilage, the fingers fitting into the corresponding intercostal spaces. Fix your elbows firmly, making them one with your sides and hips; then—

Action of Operator.—Pressing upwards and inwards towards the diaphragm, use your knees as a pivot, and throw your weight slowly forwards two or three seconds until your face almost touches that of the patient, ending with a sharp push which helps to jerk you back to your erect kneeling position. Rest three seconds; then repeat this bellows-blowing movement as before, continuing it at the rate of seven to ten times a minute; taking the utmost care, on the occurrence of a natural gasp, gently to aid and deepen it into a longer breath, until respiration becomes natural. When practicable, have the tongue held firmly out of one corner of the mouth with thumb and finger armed with dry cotton rag.

[*Note.*—Avoid impatient vertical pushes; the force must be upward and inward, increased gradually from zero to the maximum the age, sex, &c., may indicate. Abandon no case as hopeless within an hour's useless effort.]

For the Public.—In answer to requests from several medical gentlemen, I transcribe, with slight alteration, the following rules of the Life-Saving Society of New York:—

RULE I.—*To drain and force water from the lungs and stomach.* INSTANTLY place patient face downwards, a hard roll of clothing being placed beneath pit of stomach to raise it as much as possible above the level of the mouth. Put one wrist of patient

under his forehead to raise mouth off the ground. With hands well spread upon patient's back, above the roll of clothing, throw upon it your whole weight with a forward motion, and keep up the pressure about three seconds, so as to force all water from stomach and lungs out of the mouth, ending the pressure with a push which will help to jerk you back to your upright position. Repeat this once or twice, and then QUICKLY proceed with

RULE II.—*To make the patient breathe.*—Turn the patient face upwards, the same hard roll of clothing being now beneath his back, the shoulders slightly drooping over it. Bend head backward and downward, putting throat on the stretch to the utmost. Place hands of patient on top of head (one twist of handkerchief or string around the crossed wrists will keep them there). Rip or strip all clothing from waist and neck.

Now kneel astride patient's hips. Grasp front part of chest on both sides pit of stomach; your thumbs pointing to patient's chin; your fingers fitting into the grooves between the short ribs.

Fix your elbows firmly, making them one with your sides and hips; and then firmly pressing the sides of the patient together and using your knees as a pivot, throw yourself slowly forward for two or three seconds until your face almost touches the face of the patient, and your whole weight presses upon his chest. End this pressure with a short push which *suddenly* jerks you back again to the upright kneeling position.

Rest three seconds while the ribs spring back; then repeat this bellows-blowing movement as before, gradually increasing the rate from seven to ten times a minute; but take the utmost care, on the occurrence of a natural gasp, not to interrupt it; but, as the ribs fall, gently press them and deepen the gasp into a longer breath. Continue this until the natural breathing which you are imitating needs no further assistance. If all fails, keep on; because any moment within an hour's effort you may unexpectedly be rewarded with success.

[*Note.*—If a second person be present and can do it, the tongue should be held out of one corner of the mouth by the thumb and finger, armed with a piece of dry cotton or linen rag. Of course, it is understood the force employed will be moderated in both rules according to the age and sex of the patient.]

Observations upon the Directions for the Treatment of the Drowned, published by the Royal Humane Society and the National Lifeboat Institution.—Although I have for several reasons a strong disinclination for the criticism of other methods, I can scarcely conclude this paper without, in the interests of the societies whose aim I have so much and so long shared, making

one or two observations growing out of considerable experience with the directions published by the Royal Humane Society and the National Lifeboat Institution.

In the report of the Medical Chirurgical Society's Committee of 1862, Experiment 19 is cited to show "the enormous force" with which foreign substances may be drawn into the air-passages, and Experiment 39 to show that in death from drowning, as contrasted with that from simple apnœa, "the lung-substance appeared to be saturated and sodden with water." My own experience not only corroborates this as true in cases of prolonged submersion, but that in submersion for shorter periods sufficient water frequently gets into the stomach to embarrass seriously the action of the diaphragm. In neither of the published instructions referred to, however, are any directions given for the preliminary disposal in any case of this accumulation; notwithstanding, it is a frequent cause not only of serious respiratory obstruction, but which may be, as I have shown in a case of my own, sufficient to cause defeat of attempts at artificial respiration which might otherwise have proved successful.

Prominently in the first part of the instructions of both these societies is the following (the quotation is *verbatim* from the Directions of the National Lifeboat Institution, those of the Royal Humane Society being substantially the same): "Draw forward the patient's tongue, and keep it projecting beyond the lips; an elastic band over the tongue and under the chin will answer the purpose, or a piece of string or tape may be tied around them; or by raising the lower jaw the teeth may be made to retain the tongue in that position." Although fully appreciating the object of this direction, yet, after careful personal inquiries of the *employés* of these and similar societies in different countries, I have obtained but one instance of drowning in which, during the past twenty years, the first part of this rule has been followed. That instance was when a surgeon of the Royal Humane Society happened to be himself bathing at the time of a drowning casualty, and with the forceps of the institntion in Hyde-park he prised open the jaws, and, as directed, "pulled forward the patient's tongue." Regarding the remainder of the rule quoted, I have not found one instance in which either of the other directions in it have been followed. After many trials personally, I have found the procedures as directed are, for myself, quite impossible; that may help to explain the total neglect by the public and by the various societies' *employés* of the whole rule occupying such a prominent place in the published instructions. To perform the semi-rotation of an apparently dead body, or to take the necessary step forward and step backward, making besides in either method the alternate pressure directed, "about fifteen times a minute, or nine

hundred times an hour," as directed, would be a feat exceedingly difficult for even a trained operator, and one which, as I think I have shown, even if accomplished, is one in every respect perhaps of doubtful expediency. The direction further on that "the above treatment should be persevered in for some hours," everybody will agree is one which, if intended to be followed by the same person, might admit of revision. Laying aside the possible effectiveness of the methods contained in the published instructions referred to, I have had Humane Society *employés* on the Seine, Rhine, Danube, Thames, and on various coasts, practise upon my own person the methods in question. I am sorry to say that never but once, however, and that was in London, have either of these methods by any of these men been performed upon me as if the operator understood the *principles* of the method in question. We must all agree that the practical value of a given method to the public consists not only in the possible effectiveness of that method, but also in the ease with which it is likely to be understood and practised by the average layman.

Advantages of the Direct Method over other Methods in Directions issued for Public Use.

1st. It provides for ejection and drainage of fluids, &c., from the stomach and lungs, and in such a way that each motion for ejection induces an alternate inspiration.

2nd. It excludes the tongue from the pharynx without manipulation.

3rd. It secures elevation of the epiglottis, and a post-oral airway from the glottis to the nares.

4th. It includes a remedy against syncope and cerebral anæmia.

5th. It obtains a more general expansion of the thorax.

6th. It effects a thoracic compression which is more complete, better distributed, directed, and regulated.

7th. It is done by one person, and can be applied instantly on the spot wherever the patient is found.

8th. It is more easily understood by the illiterate.

9th. It is less fatiguing to the operator. In both rules there is but one motion; that it is simple, the force employed being chiefly the weight of the operator, with intervals between each motion of complete rest. As far as weight of the operator will permit, it can accordingly be performed by any person of any age or sex.

10th. Instead of probable interruption of returning respiratory efforts, aid is accurately afforded them till the breathing becomes natural.

11th. There is no unnecessary motion or violence, with its occasional dangers, at the most critical moment.—*Lancet*, *May* 25, 1878, *p.* 748.

73.—ALCOHOLIC DRINKS; AS MEDICINES, AND AS POISONS.

By Dr. ALFRED CARPENTER, Lecturer on State Medicine, St. Thomas's Hospital, &c.

The first deduction to be made from the continued use of alcohol is, that the body after a short interval becomes very tolerant of its effects, and apparently, the more, in moderation, a man takes into his system, the more he can carry without apparent injury. Those accustomed to the daily use of alcohol can take a much larger quantity with impunity than the teetotaller can do. One man may take a bottle of wine and half a pint of brandy every day of his life, making 365 bottles of wine and nearly 100 bottles of brandy every year, whilst another man only takes a bottle on a given day once a week, or three or four glasses of brandy and water on Saturday night. The one may be of the race of "fine old English gentleman," who never transgresses the rules of propriety, and goes honoured sooner or later, generally sooner, to his grave with blessings on his head, whilst the other becomes an habitual drunkard, a transgressor of the law, and an outcast of society, although he does not consume one-sixth part of the liquor which is taken apparently with impunity by the other. This result shows that conditions vary very much in different constitutions, according to the regularity with which the dose is taken, and alcohol, or some of its constituent elements, collects much more rapidly in some habits of body than in others. It differs within certain limits, from most re-agents, and helps to protect those using it regularly from its own evil effects. In this it appears to be allied with tobacco, and also with certain miasmatic influences which require some constitutions to be acclimatised before they can bear that which is manifestly injurious in its first effect, however they may be able to stand against its bad influence after a time.

It is stated by microscopical observers that alcohol acts immediately upon the blood-corpuscles as well as on the fibrine which is contained in the liquor sanguinis. It is said that one part of alcohol mixed with 500 of blood interferes with the power of the corpuscle to absorb oxygen; it abstracts water from the blood-discs and makes them adhere together in masses in the blood-vessels; it also causes the plastic or fibrinous part of the blood to coagulate, which then collects in the capillaries, and the current may be thereby stopped.

From frequent observations I incline to believe that this is true, and that this result explains some of those unfortunate cases which now and then occur to the obstetric practitioner. The free exhibition of alcoholic stimulants to the weak and debilitated, especially when there has been hemorrhage or long-

continued suffering, and perhaps too great dependence on stimulants, has led to the sudden development of embolism. A clot of fibrine has been deposited rapidly in the heart or large blood-vessels, and the patient is dead in a very short time. I have also seen many cases of paralysis and epileptiform convulsion which had been caused by embolism, and which, in my opinion, had been promoted by alcoholic saturation. Such cases have done well, and have ultimately recovered, if alcohol has been superseded; but if it has been persevered in, in most instances which I recollect there was a continuation of the disease. An excessive quantity of alcohol does impede the purification of the blood, and the elimination of carbonic acid by obstructing endosmose and exosmose in the lung capillaries; the purple tint which pervades the skin of the regular toper is sufficient proof that this is true. Observation shows that the exhalation of carbonic acid is decreased in quantity after the imbibition of alcohol.

Whilst these are undisputed facts as regards large doses of alcohol, it has not been shown that a similar result follows from the smaller quantities which are found in the weaker drinks, such as natural wines and weak beers.

Alcohol is not cumulative under ordinary circumstances, or the man who takes his bottle of wine daily would suffer more and sooner than he who only takes a bottle one day in the week; but it is cumulative under some conditions, at present unknown, and in which it is probable that the ordinary elimination or oxidation is interfered with, so that the toper, who could formerly take his bottle with ease, gets quite drunk by taking three glasses of his favourite liquor. One very eloquent writer and former orator to this Society says, that "we may trust a great deal to instinct, and that tastes are generally speaking, expressions of the wants of the system." I consider this a most dangerous argument to use, for no man, taking stimulants, can be himself aware of all the harm that they may be doing to his constitution, or know the time when they may become cumulative. All those who indulge in any kind of vicious practices would be glad to be able to rest upon such a dictum, and to affirm and excuse themselves by affirming that their vicious tastes "were but the expression of the wants of the system."

It appears to be supported by a sufficient weight of evidence that, when once the system has been exposed to the full influence of alcohol in such a way that a portion of the blood has been so acted upon that its discs have become corrugated and some of the fibrine coagulated by its action, a cumulative effect may begin, and with that cumulative effect, there will be developed a taste for more which prevents the instinctive desires of the individual being any guide as to the

real wants of the system so long as any altered material exists in the blood; the lightest kind of wines generally contain 10 per cent, of alcohol, so that they must be diluted with five times their amount of water to reduce them to a harmless condition. The reduction is seldom effected in this country, although the general consumption at the dinner-table of saline waters, which has lately come into fashion, is tending greatly in the direction indicated. Porter, stouts, and ales, all contain at least double the amount of alcohol beyond that which would make them safe for absorption at all times, unless freely mixed with other things. To take them, therefore, on an empty stomach tends more rapidly to bring about the point of saturation than when food is taken with the drink.

Alcohol transudes most rapidly through membrane, the rapidity of transudation varying very much according to the stage of dilution, and in its transudation it acts upon the membrane, or is itself acted upon. It is quickly conveyed to all parts of the body, and we have no means of judging where it is likely to manifest its first effects, neither have we any means of knowing when the limit is reached beyond which it will not be safe to allow blood disturbance to proceed. The alteration in the shape of a few blood corpuscles, the interference with their diffusive power, the interference with the transudation power of membrane, or the coagulation of a few grains of fibrine, are of comparatively little consequence, especially if the place is one of secondary importance; but when the number of altered corpuscles passes beyond a certain percentage of the whole volume of blood, evil must result. This percentage is much more rapidly reached in some habits of body than in others; it will depend very much upon the power of those organs to do their duty, whose function it is to remove the altered fibrine and imperfect oxygen carriers from the circuit. If they are defective deterioration will be much more rapid. Thus, of two men, each taking the same quantity of liquor in the course of a month, one taking it only with food and the other on an empty stomach, the latter becomes rapidly saturated, the former escapes without serious damage.

If coagulated fibrine is not removed as soon as coagulation takes place, if it gravitates to some one organ and impedes circulation there, we have the commencement of those organic changes which are constantly found in the bodies of the intemperate classes, by means of which their habits are unerringly revealed to the pathologist. The fibrine may be removed at first without setting up mischief, but the altered blood corpuscles and interference with physical power of membrane increases the evils which arise from want of oxidising power, and fatty degeneration sooner or later follows.

Here is the great difficulty which meets the view of all those who have considered the action of alcohol in an unbiassed manner, and which has led so many among us to become total abstainers. They refuse to take alcohol as a luxury, and they determine that they will not in any way encourage its habitual use among the people. The evil is so great and the resulting good so small, that they prefer to do without the good and so avoid the evil; and as society is at present constituted they are probably right. Still, the lighter wines, if entirely unfortified, and the weaker beers may be taken with impunity; they may even assist digestion and be of advantage with, or immediately after, food. If taken with other food, when great exertions are being made, and when there is a call upon the stomach for fuel to supply waste of tissues, there is sufficient evidence to show that they enable the machine to obey that call with better effect than would be the case if stimulants were altogether withheld; but if the effort is continued from day to day, beyond the ordinary capacity of the machine, and that effort is sustained by more fuel in the shape of stimulant, the human machine, like all others, must wear out sooner than it would otherwise do.

It follows, therefore, that the habitual use of strong drink even in a diluted form, for the purpose of enabling the receiver to do more work, is necessarily hurtful, unless that dilution is much less than it usually is; whilst, if the exertion is not called for, the effect must still be hurtful by setting up organic changes in the tissues of the body itself, and laying the first foundations of disease.

Whenever, therefore, the consumption of alcoholic drinks exceeds the quantity which gives rise to the probability that more than one part of alcohol to 500 of water may exist in any part of the circulation, harm, in some form or other, must result. That harm need not necessarily be permanent, and its cause may be removed altogether from the system, or it may be partially removed, a particle of matter being left *in situ*, which will act as a foreign body when alcohol is again taken, aggregating to itself fresh coagulated matter or freshly agglutinated corpuscles, by which the symptoms of alcoholic excess are rapidly induced, when those particles are situated in any of the primary nerve centres.

A stimulant in moderate doses promotes the secretion of gastric juice; it brings a flush of blood to the capillaries which surround the gastric follicles, produces congestion. This is relieved by a free flow of gastric fluid, and the rapidity of digestion is promoted. But the action is followed by a re-action, and the next day there is a deficiency of gastric juice, and as a consequence a want of appetite. The deficiency may be remedied by another dose of the irritant. The dyspeptic symptoms

are relieved, for the time being, by "pick-me-up," and so each daily exhibition leads to increased dyspeptic disturbance, until the follicles are so altered that the whip no longer acts, and the general health fails. If an excess of stimulant is taken at any one time, it acts immediately on the pepsin in the gastric juice, as well as the lining membrane on the vessels, and destroys the power of digesting and re-arranging the materials which are contained in the food.

There is no difficulty in accounting for the anorexia of the drunkard, or for the temporary flash and power which another dose of his liquor provides for him.

The rapidity of the current of blood being delayed in the capillaries of the stomach their tension is interfered with, they dilate, and a larger quantity of blood is present than is right; as a sequence, local warmth is promoted, and, in vulgar parlance, "coppers are hot." But it has been fully proved by accurate observers, that the quantity of heat in the body is not materialy added to by alcohol, but that, on the contrary, there is a reduction from the general amount in consequence of the loss which arises from increased radiation from the surface of the body. The temperature of the "dead drunk" man is lower than that which usually marks the natural heat of the body. This rule does not obtain at all times, as there are exceptional conditions which occasionally modify it. A dead drunk man often has some disease going on in his body which raises the temperature to above 98·5; but if there be no active disease, and if the temperature of the body of an insensible person is found below the natural standard without any evidence of collapse from any other cause, it may be safely assumed that he is drunk rather than that he is suffering from active cerebral lesion.

The peculiar way in which alcohol transudes through membrane, its diffusive power as it is called, enables it to reach all parts of the body in an extremely short space of time; the result of which is that the picture which I have drawn of the gastric vessels, may be found in any other organ of the body. There is dilation with reduced power of contraction. This vaso-motor paralysis, so to speak, is more continuous as well as more complete in some organs than in others, and some forms of alcoholic drinks produce it much more certainly than others. This is the case if the alcohol be impregnated with some of its allies, especially amylic alcohol and fusel oil; these latter are always more or less present in potato spirit, with which wines are commonly fortified. The influence of these alcohols is very decided upon the nervous centres; they immediately produce some of those exaggerated forms of drunkenness in which violent excitement and maniacal delirium are manifest. They

make the fluid pleasanter to the taste; and some of their forms are frequently added for the purpose of producing a so-called "bouquet." Their effects are far more serious and immediate than those which follow from the simple use of ethylic alcohol. Time fails me to do more than allude to this important point. It may be that some conditions of the body, which arise from a continuous soakage with alcohol, may enable it to change ethylic alcohol into some of the heavier forms fusel oil may be manufactured, so to speak, in the human economy itself. The odour of the breath of a dead drunk man is often more than the odour of alcohol, but upon this point chemists have not yet afforded any safe information.

It is uncertain whether the congestion arises from an immediate action upon the tissues of the capillary, or whether the first effect of the alcohol may not be on the nerve, or whether an effect upon the nerve centre may not also accrue, as well as a local effect be produced before vaso-motor paralysis can arise. It may, like the blush of shame or the pallor of fear arise from direct nerve influence, or it is possible that the blood, altered by the alcohol, may be unable to affect the nerve centre so as to exert its controlling influence upon the particular set of capillaries more immediately concerned, or it may act in different ways according as the membrane is or is not altered by the alcohol. Be it as it may in its first origin the effect is to interfere with the proper renovation and nutrition of the part, and to commence a disease of the particular organ affected. Dyspepsia is the first outcome of the use of stimulants, and as a sequence to a common result, viz., inflammation of Glisson's capsule, we may have so-called cirrhosis or fatty degeneration of the liver, and similar changes may take place in the kidney; the heart and large blood vessels may suffer directly or indirectly; the muscles may be destroyed, or the nerve tissue of the brain or spinal cord may be the parts which show the effect of the reagent first. Our periodicals teem with cases of disease which are clearly traced to alcohol as the exciting cause.

The fluids of the body may be very near to that point at which the specific action of alcohol may be made immediately manifest by setting up some symptom of nerve irritation as soon as the vaso-motor paralysis subsides; just as in the case of water, the rise or fall of one degree in temperature is physically of no moment unless it be at 32 degrees Fahrenheit, so a glass or two of alcoholic drink is not immediately of serious import unless the stage of saturation has been reached. Then an action analogous to crystallisation commences; there is an aggregation of altered blood discs or of coagulated fibrine either in the nerve centres or glandular excretory organs, or the muscular tissue, or, indeed, in any part of the body towards which it may

gravitate. The deposit sooner or later gives rise to a train of symptoms, some of which always are associated with the idea of over stimulation.

There is another class of disease not always so clearly identified with the exhibition of alcohol. I seldom meet with acute neuralgia in the total abstainer, whilst hysteria is in a great measure absent in those families whose ancestors have been perfectly temperate people. I have traced back several cases of strongly marked hysteria, and in all I have found a certain dependence upon alcohol, not only in the patient, but also in the patient's immediate ancestors. I have met with numerous cases of acute neuralgia in highly sensitive females; it is generally styled agony in those who are hysterically inclined; some declare that it is only relieved by alcohol, or chloral hydrate, or some other narcotic. The medicine allays the pain if given in sufficient doses; but, as the effect of the doses goes off, the disease returns with still greater intensity, until, at length, the stomach rejects the remedy, or some other circumstance comes into play which causes it to be laid aside. The pain has then to be borne with for a time, but after two or three days of comparative abstinence the pain subsides and the patient is in comfort. There is then, from some accidental cause a transient return of pain; the stimulant or the narcotic is again resorted to; it relieves for a short time, but that relief is purchased by a return of acute pain; there is soon afterwards another paroxysm, and the whole course is gone over again.

A close attention to numerous cases of this kind, both in high life and low life, has satisfied me that, so long as the alcohol is given, sufficiently in excess to induce moderate vaso-motor paralysis, there is relief from the pain; but, as soon as that influence lessens, there is an aggregation of altered fibrine, microscopic in quantity, either in the capillaries which supply the sentient nerves, or in the nerve cell itself, which sets up an irritation, which is felt at the periphery of that particular nerve, and neuralgia results as a sequence to that irritation.

Many kinds of so-called rheumatism have a similar origin. The imbibed alcohol affects different parts of the cerebro-spinal nervous system, according as the result attaches itself, as altered matter to one or other class of nerve-cell, and neuralgia or rheumatism, or rheumatic gout, as they are often called, or even gout itself, is the outcome of the action.

The cases of neuralgic disease, to which I more especially refer, occur in highly hysterical constitutions, and are not generally accompanied by any cutaneous capillary congestion, except when neuralgic pain is absent. There is sometimes great mental power in this class of cases, a brilliant intellect or great genius, which only shows itself when the cerebral capil-

laries are congested, and the cutaneous system gorged with blood. These cases generally go from bad to worse, and end in suicide, or as general paralysis or dementia. The tissue of the brain and spinal cord being subject to changes which correspond, in a great measure, with those diseases of liver and kidney which are induced by alcohol, a low form of inflammation is set up first, and after that atrophy from impaired nutrition or fatty degeneration, ending in so-called white softening.

There is also a class of cases, not so generally attributed to alcohol as a cause, which I believe to have their origin in its habitual use, which are due to spinal irritation, and are sometimes called spinal neuralgia.

The cases in which the temporary administration of alcohol may be useful are those in whom the surface of the body has been chilled, and in whom the powers of life are weakened in such a manner that the heart is unable to do its work of propelling the blood to the capillaries with its usual ease. The action of alcohol in these cases is like taking off the pendulum of a clock. The spring is able to work so much easier and to get over an extra amount of detail in the same space of time. Internal congestion, irregular circulation, so to speak, fulness in one place, with unfilled cutaneous capillaries, are cases in which alcohol may be exhibited with immediate advantage. The tension of the capillaries being overcome, there is a diminution of the impediment to the flow of blood and, as a sequence, a possible diminution of the internal congestion. So if the action of the heart itself is too weak to overcome the tension of vessels, we may induce a kind of paralysis to enable it to do its work more easily for the time being and get over a passing difficulty in that way. How far it is prudent to push this for any length of time is another matter, as a continuance of it must lead to the first stages of tissue change, which then becomes itself of primary importance.

It appears that we may use alcohol with advantage, in moderation only, in cases of prostration from shock, and the prostration which follows acute diseases after such have expended their force, but that its continuance as a remedy is fraught with danger to those who take it.

There is a class of persons to whom alcohol may be useful even in comparative health. When the powers of life begin to decay, when the force of the heart is not enough to transmit the blood to the extremities of the body, and the aged person easily feels the influence of cold. The blood scarcely passes through the unfilled cutaneous capillaries. By dilating them and diminishing their tension the heart is relieved and the

functions of the skin and other organs are more efficiently performed. A moderate dose of alcohol taken with food is always beneficial to an old person under such circumstances, and I cannot see any reason why it should not be administered. Again there are conditions sometimes present in youth, in which alcoholic drinks also are useful; no other kind of stimulant acts so equally and so satisfactorily; there are a few cases, and a very few only, in which they may be administered in middle-age; taken as a whole, a weak heart with unfilled cutaneous capillaries will be always found in those cases in which it is likely to be beneficial. The stethoscope most clearly reveals them, the impulse of the heart and the general rhythm of the pulse not at all corresponding.

So long as alcohol is taken only in such quantities that it becomes utilised at once in the equalisation of animal heat, no harm can result, but if there is more than sufficient to do this, and its specific action is produced on the blood itself, mischief sooner or later results. In this it differs from fat, with which it seems to be allied as a diet, which, if not used up, may be stored in its proper place out of the way, but the changed matter which arises from an excess of alcohol is always deposited in the tissue of an organ. Thus its effects may find a home in the capillaries themselves, interfering with their elasticity, or it may be laid down in muscular tissue, and there is then a decrease of power and so-called rheumatic pains are produced. If nerve tissue is the seat, there is interference with the power of thought, with volition, self-control, and other mental actions, the effect of which is to render the patient a weak-minded and unstable character. In health it does not prevent the waste of tissue, whilst it does prevent the discharge of effete products. There is a decrease of the formation of carbonic acid, and in this is probably found the reason why there is a decrease of temperature. It stands in the way of more beneficial actions unless it be at once used up. If there is no muscular exercise going on, or no extra mental labour called for, and no action at all required except that which keeps up temperature to a normal standard, it is hurtful; work which is kept going by continual doses of alcohol always ends in a break-down. If it is taken for the purpose of increasing muscular exertion, ultimately there is great loss of muscular power, as athletes well know. If mental exertions are kept going by alcohol, there is mental break-down, as the lunatic asylums testify; whilst it is quite impossible for organic life to be performed in a healthy manner if alcohol is habitually used to promote digestion. The evidence which was afforded by the Abyssinian and Ashantee campaigns and the Arctic expedition help to prove this, whilst the unanimous opinion of those who

are competent to judge informs us that in the Russo-Turkish war the temperance of the Turkish soldiers enabled them to bear injuries with impunity, whilst similar injuries easily caused the Russians to succumb.

It has been argued by some experts that it is unwise to leave off alcoholic drinks at once, that it is better to do it by degrees. I never could see any reason for this. The experience of all gaol surgeons is in accord with my idea, that no evil, but real good, results from immediate and total abstinence. I am satisfied that when mischief is arising from its use, it is best to immediately desist from it. If mischief then results it is because it had already commenced, and not from the abstinence; although when a man has been accustomed to his daily dose of alcohol for a long series of years, and his constitution has become acclimatised without evidence of tissue change, he should not abandon its use. I could not advise an old man to give it up; such a change would certainly do harm, not good.—*Medical Press and Circular, May* 15, 22, *and June* 5, 1878, *pp.* 400, 419, 462.

74.—ON AMYL NITRITE AS A CARDIAC STIMULANT.

Dr. James L. Minor, of Rapidan, Va., in the Virginia Medical Monthly, states that little need be said in regard to the action of amyl nitrite upon the heart, for frequent experiments have proved beyond dispute the vigorous action of that drug upon the centre of circulation—yet experiments may prove interesting, although they may simply illustrate an established doctrine. A patient who had locomotor ataxia of some years' duration was suddenly taken while under Dr. Minor's charge, from a condition of usual health to that of impending death. There was the Hippocratic expression well marked; general pallor, with complete unconsciousness. Respiration was stertorous and irregular. Arterial beat imperceptible at the radial pulse, but faintly recognised at the femoral pulse. The pupils were insensible to light and somewhat dilated. The extremities and exposed parts of the body became cold and clammy. It was decided that nitrite of amyl should be tried, but with no idea of permanent relief, for the patient was too evidently fast approaching his end. The ordinary method of inhaling a few drops from a piece of muslin was adopted; no effect being produced, recourse was had to a hypodermic syringe, with which three minims were injected. In a few moments the heart responded, as evinced by the appearance of a more natural hue of the cutaneous surface. The pulse was recognised in the radial artery. Respiration became much better, and the temperature felt normal. The effect of the medicine seemed to

pass off in about half an hour, when the drug was again administered—this time giving five minims hypodermically, The action was similar in kind, but more vigorous in character than the former dose. The pulse became almost as strong as normal. The patient lasted for nearly twenty-four hours, during which time amyl nitrite was frequently given. As much as fifteen minims were given hypodermically at one of the doses, when the pulse became incompressible. So long as the vital powers were able to respond the administration of the medicine was followed by reaction which seemed marvellous. This proves the powerful action of amyl nitrite, and the beneficial result which might follow its administration in certain cases calling forprompt cardiac stimulation.—*Practitioner, Sept.* 1878, *p.* 218.

75.—ON THE TREATMENT OF GOUTY HEART.—FATTY DEGENERATION.

By Dr. J. MILNER FOTHERGILL, Assistant Physician to the City of London Hospital for Diseases of the Chest, Victoria Park, and to the West London Hospital.

Of the gouty heart in its usually long career, it is evident that it and its outcomes are largely amenable to treatment. A great deal may be done to delay its course, and to keep off for long what cannot be remedied when once instituted—viz., heart failure. Indeed, it is well to divide the subject into two sections—the treatment of the hypertrophied stage; and the treatment of the later stage of heart failure from fatty degeneration of its muscular structure. It will be seen that the treatment of these two stages is essentially different in many respects, because it has to meet altered conditions. In the early stage we have to treat a gouty individual who is the subject of vascular complications, and the treatment must have special reference to his individual requirements. We have seen that it is the waste of his nitrogenised food which is causing the obstruction in the arterioles and raising the blood-pressure in his arteries; this, again, leads to a higher internal pressure upon the heart-walls, and this to hypertrophy of the left ventricle; and then, again, this maintains the high arterial tension, and causes atheroma of the arteries; while two distinct dangers are associated with this condition—apoplexy and angina pectoris. Both are due to the same cause, viz., sudden contraction of the arterioles, with a corresponding rise in the arterial tension; and then one of two things happens—either the hypertrophied ventricle bursts an atheromatous artery, or the heart itself becomes embarrassed, and can scarcely contract and empty itself in the face of the opposition offered by the high arterial tension. The permanent condition and the passing con-

dition—the latter fraught with danger—are alike due to a high internal pressure within the arteries. It is obvious, then, that the treatment must be directed accordingly. The dietary should contain but a small proportion of albuminoid material—indeed, just so much as will meet the tissue requirements of the body, and no more. And that is much less than is usually imagined. This is demonstrated by the well-known history of the Abbé Cornaro, who took but twelve ounces of food (chiefly vegetable matter) and fourteen ounces of light wine daily for fifty-eight years, during which time he had excellent health, and his activity, mental and bodily, was greatly increased. Without supposing that every gouty person is an Abbé Cornaro, still it may be said of them that they usually assimilate all they eat, and therefore do not require a very liberal dietary; though they may, and commonly do, allow themselves liberal quantities of food, and that too of a generous nature. Every particle of nitrogenised material taken in excess of the tissue-needs is so much more waste matter to be dealt with—so much more added to the excrementitious matter which has to be got rid of, or which, if not got rid of, will do definite and intelligible mischief. We have abundance of evidence to show that not only does nitrogenised waste raise the blood-pressure in the arteries; but we know from the observations of the late Professor Parkes at Netley, that a strictly non-nitrogenised dietary notably lowers the blood-pressure. Consequently, if the gouty patient be placed upon a non-nitrogenised dietary, all these grave and serious consequences of persisting lithæmia will be delayed and kept off for years. If the patient will persist in a dietary rich in albuminoids, an impetus will be given to a long series of pathological changes as definite as they are grave. Even if albumen be present in the urine, its presence does not indicate a liberal supply of albuminoid food to meet the drain; indeed, often the reverse—the albuminuria is kept up by the food, and is benefited by a non-albuminoid dietary. But in the renal changes associated with the gouty heart, albumen is only fitfully present in the urine, usually absent for long periods together, and only present in small quantity, when present at all, until the last stage of heart failure is reached, and then it is persistently present from venous congestion, but even then not in all cases. The patient then should be put upon a dietary which should consist of farinaceous material, fruit, fat and fish. Fish of course contains nitrogen like all other flesh, but a gouty person may eat a good meal of fish, and enjoy it, without getting any great excess of azotised material. Then he should take a good amount of vegetables of all kinds—in winter, potatoes, turnips, carrots, winter greens; in spring, spinach, but not much asparagus, for that vegetable contains a

quantity of oxalates which tease a gouty system annoyingly. Consequently, rhubarb is to be avoided; and salads, which should form a portion of the daily dietary of the gouty all the year round, as far as possible, should never contain sorrel, the *Rumex acetosus*, on account of the oxalates present in it. Greens of all kinds, except those just mentioned, cooked and uncooked, should form a considerable portion of the dietary. A lobster salad is the typical food of the gouty man whose digestion is perfect. In those gouty people who are also dyspeptic, such food is inadmissible and the dietary is a limited one. But dyspepsia usually protects the system of the gouty, and in fact takes care of it. The gouty dyspeptic usually attains a good old age. In those whose hearts are dilated as well as hypertrophied, it is well to see that the food does not produce flatulence, which is not only uncomfortable, but is not without danger, as the elastic gases press upon the heart through the thin diaphragm, and impede its action, as well as to a certain extent opposing the filling of the chambers during diastole. So much for the dietary.

Next as to the medical treatment of the first stage of the gouty heart. The radical treatment must be the getting rid of the nitrogenised waste, which persists in the form of uric acid, by converting it into a soluble salt so that it may drain off in the urine. Potash, or lithia, and the acid phosphate of soda, all form uric acid into soluble salts. They may usually be given persistently without harm. Some persons are very susceptible to the depressant action of potash, which as a muscle-poison lowers the activity of the heart, and consequently cannot take it in any quantity. But most gouty persons are all the better in every way for the steady use of potash, with or without a little lithia, and can take it freely without inconvenience. It may be given with colchicum or buchu; buchu forming an excellent vehicle. Each dose should be taken about half an hour before a meal, and be washed down with a tumblerful of water. The water is a therapeutic agent of no mean value in these cases. It dilutes the potash and washes the tissues. The first dose ought to be taken on getting out of bed, so that the alkaline fluid may bathe the tissues before any food is taken. Where the heart is dilated and weak, or there is the peculiarity present of intolerance of potash from its depressant effects, it is well to add a little nux vomica or digitalis, or small doses of both, to the potash mixture. Very often a condition of palpitation, which is only aggravated by digitalis and iron, yields readily to potash and buchu. The digitalis heightens the already high arterial tension, and causes the heart to have a hammering action which is very unpleasant to the patient; the potash, by getting rid of the exciting agent,

the uric acid, permits the circulation to quiet down to its normal point. The dyspepsia of gout yields also to a course of alkalies. Where there is great itching of the skin, it will be relieved by washing it twice daily with a warm and strong solution of bicarbonate of soda with a little potash. In conditions of anæmic or asthenic gout it is well to first give alkalies alone, and afterwards to combine them with iron, as the bicarbonate of potash with the ammonio-citrate of iron. Iron alone rarely suits these patients. Perfect depuration of the blood here improves its quality more efficiently than the administration of iron; and it is only after such depuration has been achieved that iron is assimilated properly and is useful.

Then we come to the use of agents which act directly upon the nerve-centres of the circulation, of which the most marked is undoubtedly digitalis. This potent remedy may be so used as to do harm as well as good. When the hypertrophy is good and the pulse firm and incompressible, then it is not indicated, and may be injurious. As long as perfect compensation is attained by hypertrophic growth, digitalis is not required. But it may be desirable to give it even with great hypertrophy, if, after all, that hypertrophy is insufficient for perfect compensation. The only rule that can be formulated is this one—the more the hypertrophy, the smaller the dose of digitalis should be; the more the dilatation, the larger the dose. It is more often indicated in the case of women than in men, because women less rarely have pure hypertrophy than men, and more frequently have a condition of obvious dilatation present. Whenever there is dilatation blended with hypertrophy, then digitalis is indicated; it may be added to the potash mixture with advantage. In the failure of hypertrophy, when dilatation is setting in from the walls becoming weakened by structural decay, then digitalis may be given with advantage, and will slow the downward progress even when it cannot be arrested. It should be given steadily away in moderate doses, without fear of any accumulative action. There is no more accumulation about digitalis than there is about strychnia, belladonna, mercury, or iron. If any of these agents be taken into the body in larger doses, or more quickly than it can escape out of it, an accumulation must take place. But any idea that digitalis lurks about the tissues until it gathers up and reaches a lethal or even dangerous point suddenly, that it is malicious by design, must be abandoned. It is well, however, to push it steadily in moderate doses, so that it is always present in the blood, but never in any great quantity at a time.

In many cases it is well to combine it with small doses of strychnia, and the two will be found to go well together,

especially where there is any lung or bronchial complication present and any pulmonary dyspnœa. In those cases, which, however, belong rather to the later stage of the failing heart, where attacks of uræmic dyspnœa come on during the night, and which are apparently due to failure in the respiratory centre in deep sleep, and the patient wakens suddenly out of deep sleep, in an attack of dyspnœa, the addition of strychnia to the digitalis is most useful. In some rare cases belladonna suits a patient where digitalis has disagreed, and may with advantage be substituted for it. It is by a combination of means that this first stage of the gouty heart is to be treated—a non-nitrogenised dietary, alkalies, fluids, and iron and digitalis in limited quantities. In addition to these, exercise and improved oxidation are useful adjuncts. Exercise does not tax the heart injuriously so long as its tissues are sound; neither does it provoke palpitation, as is the case with the muscularly feeble heart. The removal of waste by increased oxidation is a desirable matter. By such means the gouty heart may be long preserved and maintained, and the risks of apoplexy and angina avoided. As to the treatment of the apoplexy when it occurs, we cannot discuss the subject here; but as to angina, the inhalation of amyl during the attack is certainly indicated. Nitrite of amyl dilates the peripheral vessels, whose contraction is the starting-point of the attack of angina, and so lowers the blood-pressure in the arteries and relieves the heart. Angina is not very serious when the heart-textures are sound, but when they are decayed it is fraught with great and immediate danger of complete arrest in diastole. Recurring anginose attacks in comparatively young people often readily yield to a course of potash. I am inclined to think belladonna the remedy where angina is frequent, rather than digitalis, it the heart be weak or dilated.

But sooner or later the heart begins to fail, and then the aspect of the case alters materially. The renal characteristics yield to those of cardiac failure, which become the prominent features in the later stage of the gouty heart. The bulk of urine becomes markedly altered. The pale urine of light specific gravity, voided so copiously, gives way gradually to the scanty urine of high specific gravity, and laden with lithates—and often at last permanently albuminous, of heart failure. The aortic second sound is still distinct and accentuated, but the first sound and the apex-beat are much diminished, and there are the subjective indications of waning power in the heart. When this stage arrives the treatment undergoes a change. The question now becomes that of conserving the failing powers—indeed, keeping the patient alive. He has passed the risks of apoplexy to drift into the more certain

and graver perils of heart failure from decay of its muscular walls. Angina is more dangerous now than before. The failure of the heart in diastole is the danger, which, first looming in the distance, becomes ever nearer and more distinct, until at last it is absolutely present, and life closes. It is a downhill fight all the way until the inevitable end is reached; but it does not follow that a fight may not be maintained up to the last. It is now necessary to give digitalis and strychnia steadily and persistently, for the rhythmically discharging centres which preside over the circulation and the respiration are in danger of failing, as fail they will at last. Exercise becomes impossible, and improved oxidation is only attainable by passive exercise in a carriage or a bath-chair. Walking becomes painful and distressing.—*Medical Times and Gazette, Nov.* 23, 1878, *p.* 591.

76.—PHOSPHATES IN NUTRITION, AND THE MINERAL THEORY OF CONSUMPTION AND ALLIED DISEASES.

(From a Review of Dr. M. F. ANDERSON'S Work on "Phosphates in Nutrition.")

The idea which Dr. Anderson mainly elaborates is this: that the *inorganic* materials in the soft tissues, and their relative proportions, exert a very important influence in nutrition; and that their absence or presence in insufficient quantities, either from diminished supply or imperfect assimilation, is the origin of a class of diseases (organic) which have hitherto received no adequate explanation as to their cause. He believes that the only form of phosphorus met with in food and in the animal tissues is phosphoric acid,—although some chemists represent the Protein compounds as containing phosphorus either in an elementary state or in some other combination than phosphoric acid. All the phosphates in the tissues are tribasic; nervous tissues containing in addition phosphoric acid as a hydrate or in combination with organic matter. Those tribasic phosphates possess the peculiarity, that the combination between the base and the acid need not be confined to one particular base; that is to say, the three equivalents of base may be built up of two, three, or more different bases, but always containing three equivalents of basic matter to one of acid. Take tri-calcic phosphate as typical of this class; we may substitute for two equivalents of lime, one of magnesia and one of soda. There are here three equivalents of base to one of acid, just as in the tri-calcic compound. Each of these separate bases may be again divided and represented by an equivalent portion of some other base. These tribasic salts, Mr. Anderson tells us, are insoluble in alkaline solutions, sparingly soluble in neutral; and this deserves being noted. They are of great importance in the

formation of certain tissues, and their existence can be made out clearly in all tissues and organs which contain capillaries. He infers that they are components of arterial and capillary membrane, more especially as the inner membrane of arteries can be shown to contain them. He gives an interesting table showing the quantities in which phosphoric acid has been yielded by vascular tissues and organs, such as the skin, liver, kidney, lung, brain, &c.; while tendon, which is sparingly vascular and contains much less water than the other textures, exhibits a mere trace of phosphoric acid. This seems a strong confirmation of his theory, that the seat of phosphates in the tissues is in the capillaries. Why not bring it to the test of the microscope? Because that instrument takes cognizance of the minutest details of structure, but cannot as yet reach so far as chemical composition.

The capillaries, including arterioles, capillaries proper, and venous radicles, have to do with the building up of tissues; and it is essential to keep in view, that the inorganic constituents of food, the phosphates among the rest, are as essential for perfect nutrition as the flesh-formers and heat-producers,—the Protein compounds and the hydro-carbons. This must be obvious to every one who duly considers the subject. Mr. Anderson treats this aspect of it with great care both as regards the *ingesta* and the egesta, and arrives at the conclusion, that much light is thrown by a deficient supply of phosphoric acid upon some very important diseases—scurvy, rickets, scrofula, tubercle, and cancer.

Scurvy.—We have already seen, that what may be called tissue phosphate takes an important part in capillary formation, and a consideration of the conditions which produce scurvy, the symptoms of the disease, and the treatment, illustrate the theory of capillary construction advocated. The late Dr. Parkes had almost come to the same conclusion, for he says: "It seems clear that it is to the absence of some of the constituents of the fourth dietetic group, the salts, that we must look for the cause." The conditions favouring an outburst of scurvy are, want of wholesome fresh meat, of fruit, and succulent vegetables, accompanied generally with the use of salt meat. When meat is salted it loses a large quantity of its phosphates as well as a portion of its albuminates; and the phosphates abstracted are of necessity the most soluble, the most easily assimilated, and, in consequence, the best tissue-formers. Two specimens of fresh meat were found to contain respectively, ·256 and ·320 per cent. of phosphoric acid; while a sample of salt-beef from the stores of H.M.S. Hector, while in commission, showed only ·147 per cent. of phosphoric acid. The salt meat, thus examined, had not been previously soaked; but before being used in the

Navy, it is soaked for some time in water to remove the brine, the effect of which will be, without doubt, to remove a still larger quantity of the phosphates, so that, when ready for consumption, it can contain but little phosphoric acid or phosphates. *Pemmican* ought to be of great use, containing, as Dr. Frankland shows, five or six times the quantity of phosphatic salts that salt meat contains; but in order to that, the phosphates must be soluble, or be rendered so by the use of citric acid. How does lime-juice act as an antiscorbutic? In virtue of the citric acid which it contains in the proportion of 5 to 7 per cent. The other ingredients, potash and phosphoric acid, in lime-juice, can have no appreciable effect; they are in such infinitesimal quantities—·22 and ·30 per cent. The other articles of diet on shipboard contain both in far larger quantities; the biscuit ten times as much potash, and phosphoric acid quite sufficient for dietary purposes, if it were in solution, or capable of being rendered so by a proper solvent. "The antiscorbutic action of lemon-juice is thus reduced to its citric acid, and the efficacy of this depends upon the conversion of insoluble phosphates into soluble for the formation of tissue phosphate."

Mr. Anderson gives an interesting account of the chemical actions of citric acid on the tribasic phosphates, as contrasted with those of the mineral acids, and sums up thus: "These facts, therefore, as regards the action of citric acid, together with the doctrine of capillary nutrition, tend to show that the efficacy of lime-juice as an antiscorbutic depends mainly on the presence of citric acid, and is referable to the solvent power of this acid on phosphates."

How is this reconcilable with the view entertained by Ganod and others, that the deficiency of potash in scurvy is the important circumstance? In this way, potash being the base which, of all others, exists in large quantity in the inorganic portions of food, if it be deficient, we may take for granted that the other ingredients, phosphoric acid among the rest, for the formation of tissue phosphate are also wanting; but in their absence in any easily assimilated form, to give potash alone as an antiscorbutic is useless.

Again, if the blood, by the agency of acids, particularly the mineral acids, be rendered abnormally acid, tissue phosphate is not formed. The bases which should go to the formation of this salt are combined with the stronger mineral acids, as chlorides, sulphates, &c., and cannot be assimilated in this soluble condition. The same results occur as if there was an absence of material for tissue-phosphate formation. The symptoms of innutrition are identical. It matters not whether it be from absence of proper material for tissue-formation, or from non-assimilation, whatever the cause of this last may be.

Had space permitted, we might have shown the application of the author's views to the various wasting diseases already enumerated; but we must confine our remarks to one.

Mr. Anderson holds, that, as regards *consumption* we must look in the capillaries for the first radical changes which produce all the varieties of that disease, and that the defect consists essentially in defective mineral nutrition. He recognises in advanced cases another agency which seems to exert considerable influence in preventing recovery: *septic influence*. Disintegration of tissue brings the effete products of tissue destruction within reach of the absorbents or of the blood, and so has a tendency to vitiate the blood. No one doubts this in cases of advanced cancer; is it otherwise in phthisis? The general emaciation shows that the whole system is affected by the disease; the nutrition of the whole body is interfered with. Such cases may be confounded with cases of starvation. Is there anything that may decide betwixt conflicting medical opinions, as in the recent Penge case? Here is an interesting and important answer: "In starvation, the mineral constituents of tissue-phosphate would not be materially lessened,—from wasting of the tissues caused by the combustion of their organic matter, there would be an apparent increase in the percentage proportion of mineral matter; on the other hand, in tubercular disease there would be a material decrease in the mineral matter as compared with the general wasting. In starvation, the soft tissues are quickly burnt up to supply heat, before the capillaries have time to suffer any very material change in their mineral composition; in tuberculosis, the capillaries are the first to suffer."

In consumption and other wasting diseases, the capillaries are underfed, and cease to do their work. This may arise either from deficient supply or from non-assimilation of mineral matter. It is the starting-point of the disease, and life ebbs out slowly. In starvation, from want of food generally, death ensues whenever there is an insufficiency of heat-producers in the blood or the tissues to keep up the normal temperature. Under such circumstances, the remedy is at hand, if it be wisely and cautiously administered. But in organic disease, such as phthisis, the treatment adopted may slowly but surely do harm. A dietary deficient in the one necessary ingredient may be chosen, if we are guided merely or chiefly by the attenuated state of the patient. Cream, cod-liver oil, and other food rich in the heat-producing element, have been often very useful in the treatment of consumption. Their use is quite explicable on the mineral theory advocated by Mr. Anderson. Capillary weakness, from insufficient renewal of its mineral components, is the first cause of the disease. These weakened capillaries fail to do their work efficiently and thoroughly; an insufficient quantity of food is, by

the process of digestion, rendered available for tissue nourishment and repair, and for the production of heat. The result is, that when all the fat in store has been consumed, muscle begins to waste in order to maintain vital temperature; or, to use the author's graphic mode of stating the case, "the stock in the coal-cellar runs low, and the house is pulled to pieces to furnish fuel for the ever-burning human furnace." Cod-liver oil, being by many persons easily assimilated, supplies fuel at the right time, and so far lightens the work of the underfed capillaries, so that what remains of their force may be turned to other purposes of nutrition. But cod-liver oil does not contain the elementary principle which the chemistry of consumption shows to be deficient.

The phosphorized oils, although containing phosphoric acid, are sometimes useful, but can do little as tissue-formers, for, without the base, tissue-phosphate cannot be supplied.

The hypophosphites have been useful in many cases; but, as hypophosphorous acid has no place in tissue formation, it can only be made use of in the system when converted into phosphoric acid. The base with which it is combined, generally lime or soda, can then help to form tissue-phosphates, the requisite compound. Keeping these principles in view, we can see how phosphoric acid, given along with the hypophosphite of soda, may add to its efficacy, and also how Dr. Charteris of Glasgow finds advantage from giving the hypophosphite of lime and soda alternately. Would it not be more reasonable, as Mr. Anderson has done, to use a tribasic phosphate containing all the bases which analysis has discovered in the coats of the aorta and all the organs examined? He proposes, by supplying all the mineral ingredients of nutrition, to leave nothing to chance, so that if there be a fair amount of assimilation, all the mineral elements for tissue-fabrication or renewal may be present.

We hope that this necessarily imperfect account of the book under review will awaken an interest in the subject, and induce our readers to study it for themselves. Many suggestive thoughts are thrown out, which may be prosecuted with good results.—*Edinburgh Medical Journal, Nov.* 1878, *p.* 449.

77.—ON THE ACTION OF STRYCHNIA AS A TONIC.

By Dr. T. Lauder Brunton, F.R.S., Assistant-Physician to St. Bartholomew's Hospital.

A most valuable tonic, strychnia has an action more widely extended over the body than digitalis. It is at once a gastric, vascular, and nervous tonic, it aids digestion like other simple bitters in the way already described. It has, with the exception of quinine, a more powerful action than most other bitters in

preventing putrefaction. It excites the sensibility of the va motor centre, thus exerting a beneficial effect upon the circu tion, and likewise directly stimulates the nervous tissue of t spinal cord itself. So great is its effect upon the vaso-mot centre that by its means physiologists have discovered th instead of being confined to the medulla oblongata as w formerly imagined, this centre extends down the spinal cor It has just been said that an impression made upon the sensor nerves, reflexly stimulates the vaso-motor centre, contractin the vessels and raising the blood-pressure, but when a cut i made across the spinal cord just below the medulla oblongat this result is not produced. From this experiment it has bee concluded that the vaso-motor centre was entirely confined t the medulla oblongata above the place of section: but if a little strychnia be now injected into the veins of an animal in which the cord has been thus divided, and a sensory nerve be then irritated, the vessels will contract and the pressure of the blood will rise. It thus becomes evident that the vaso-motor centre extends down the cord from the medulla, but that its spinal portion is so feebly developed that under ordinary circumstances it has no power to contract the vessels when reflexly excited by stimulation of the sensory nerve. But strychnia has the power to increase its excitability so much, that reflex stimulation in this way will produce through it a decided effect. Now when we consider that sensory impulses are proceeding every moment from the skin to the vaso-motor centre we can readily perceive how a slight increase in susceptibility produced by strychnia will have a wonderful effect in raising the tone of the vessels, and aiding the circulation. The mode in which quinine acts is not so clear, but we know from observation, that, also in small doses, it renders the pulse stronger and less compressible.

Tonics may increase the quantity of nutriment taken into the body generally; by their action on the vessels they quicken the circulation of inter-cellular fluid in the tissues, and by thus aiding its oxidation, and removing the products of waste, they greatly increase the functional activity of the various organs of the body.

We have now to consider how they effect the removal of waste from the body generally. The inter-cellular fluid in which these products are contained is absorbed into the general circulation by the veins and lymphatics. Unless some provision were made for its removal, it would soon accumulate in the blood and arrest the functional activity of the various tissues, beginning with that most susceptible of all, the nervous tissue, causing death. But these substances in all probability undergo further oxidation in the blood after their absorption and before

they are finally excreted. This oxidation will be assisted if the respiratory movements by which oxygen is taken into the lungs are rendered deeper and more frequent, and also if the blood itself should require greater power to absorb this oxygen. Now strychnia has an action upon the respiratory centre in the medulla oblongata similar to that which it exerts upon the vaso-motor centre, and under its action respiratory movements become both quicker and deeper. No such effect is produced on the medulla by such a tonic as iron, but under the influence of this remedy the blood corpuscles not only become greatly increased in number, as was shown by Dr. Gowers in a recent paper in the Practitioner, vol. 20, page 1, but they also contain a great amount of hæmoglobine. Oxygen is thus more rapidly carried from the lungs to the tissues and the process of combustion can thus go on more readily, both in the tissues themselves and in the minute blood vessels into which the products of waste have been absorbed. The rise in blood-pressure which occurs under the influence of the tonic not only affords, as we have just seen, the most favourable conditions for oxidation in the tissues and for the removal of the products of waste from them, but it also assists in their elimination from the body itself. It has been shown by Ludwig and his scholars that the secretion of urine is, generally speaking, proportional to the pressure of blood in the renal glomeruli, and thus the pressure would rise along with the tension in the vascular system generally. The contraction of the vessels which tonics produce will therefore raise the tension in the kidney as well as in other parts of the body, and thus aid in the elimination of the products of waste.

From what has just been said, then, it would appear almost that strychnia or nux vomica is one of the most valuable tonics which we possess. When combined with nitro-hydro-chloric acid it is perhaps one of the most efficient remedies that we can give for the debility which is so often noticed in warm weather, and when the ordinary tonics, such as gentian, columba, cascarilla, or quinine do not produce the desired results, the addition of a little nux vomica or strychnia to them may give us the wished-for effect.—*Practitioner, Aug.* 1878, *p.* 91.

78.—SULPHATE OF MANGANESE IN LIVER DISEASE.

By Dr. R. H. Goolden, London.

Any addition to our useful drugs is naturally of interest to our working profession, and the more so when we know exactly what we are using and can be tolerably certain of a constant result in its use for definite purposes. We have lately had additions to our Pharmacopœia of considerable value, such as

podophyllin, a substitute for mercury, but as it is not always to be depended upon, I shall be obliged if you will allow me to call the attention of practical men to the use of sulphate of manganese. I have been in the habit of using it for more than thirty years, both in hospital and private practice, with uniform results, to be depended upon at least as much as any medicine at our command. I first used it at the old Dreadnought hospital-ship, and published a short account of it in some of the journals of that period (1840), giving some cases that illustrated its invariable action upon the liver and its secretion. I was first led to adopt it from having constantly numbers of liver-disease cases brought to the hospital from the tropics, where calomel had been used to the extreme limit of large and frequent doses such as modern practice has quite abandoned. Podophyllin was then unknown, and all other bile stimulants were either too slow and too weak in their action to be of any use, or attended with some other properties which made them even dangerous. About that time Pereira's work on "Materia Medica" was published, and I was struck by his account of the effect of sulphate of manganese; it was short and had reference to the poisonous effects of manganese upon animals. Two drachms proved fatal to rabbits, and on examination after death, the stomach and intestines were found filled with pure bile, all other contents having been expelled by purging and vomiting. No lesion was discovered, no congestion of any viscus, but the intestines were all stained with bile. I tried the effect of small doses upon myself, beginning with one grain, and increased up to half a drachm before producing any sickness. Perhaps the gradual use of it enabled me to take a larger dose than if that quantity had been commenced with. I then gave it to such patients as there was reason to believe had no abscess or dysentery; but where there was enlargement, with either dark bilious evacuations or bileless and pale, no effect was observed for half an hour, and then they suffered from nausea, followed by purging or vomiting, or both together. The nausea was considerable, but soon passed away, leaving the patients in better spirits, with improved appetite. The dusky yellow or sallow skin became natural in colour; the disappearance of jaundice was very remarkable. I never observed any bad effect from its use—excepting the temporary nausea, closely resembling sea-sickness, and reminding me of the sickness resulting from abscess of the brain—beyond that induced by our ordinary emetics, and the relief counterbalances all the distress, which is but transient, neither the heart's action nor the pulse being altered. After the second or third dose it occurs not, and I found that the addition of small doses of Epsom salts opened the bowels, lessened the vomiting, and, in many cases relieved

it altogether. I have found this most useful in cases of hypochondriasis and hepatic dropsy. Half a drachm is the utmost limit, and, saving in cases of urgency, by no means desirable. From ten grains to a scruple will produce large bilious evacuations. Ten grains will rarely be attended with nausea, and is sufficient for ordinary purposes. I have known it often to relieve hæmorrhoids, and those congestions of the fauces and bronchia so common in our daily practice; in fact, wherever we can trace these congestions to a defect in the portal circulation, the manganese is a good substitute for calomel, as it produces no more irritation to the intestinal or anal glands than is caused by a flow of bile, and it is attended with neither griping nor depression. Five-grain doses may be repeated as an alterative instead of blue pill or grey powder, and it is remarkable that many anæmic patients who cannot take any of the preparations of iron can do so if combined with a few grains, say from two to five, of sulphate of manganese. I find the best way to administer it is to dissolve ten grains in a tumbler of water, and then add some citrate of magnesia or potassa to the solution, and drink in a state of effervescence. The taste is not unlike that of Epsom salts, but not quite so bitter, and is scarcely perceptible when taken in a tumbler of effervescing mixture. There is a slight objection in adding it to a mixture. as the property of easily absorbing or parting with oxygen will cause it to change colour if kept in solution for many hours, and the patient is apt to think that the chemist does not always send the same mixture, though I am unaware that this circumstance is of importance medicinally. This is the more remarkable if sugar or syrup be added to the mixture. In hospital practice this is of little consequence. Another drawback to its use is that, not being in our Pharmacopœia, the druggist is apt to think that "manganesiæ sulph," is intended for Epsom salts; and, being so seldom used, it is very rarely kept in stock except by the principal druggists. I have been frequently much disappointed by observing the absence of any results in private practice, and that magnesia has been substituted for manganese; but when the mistake is discovered and rectified, the proper results are produced. I therefore usually, in prescribing it, *mark* the manganese, and add "magnesiæ sulph." to call the attention of the dispenser to the difference. I believe this to be one cause why it is not more generally used. I have myself shown its use, both in clinical lectures and hospital practice, for over thirty-five years, at both the old Dreadnought and St. Thomas's Hospitals.—*Lancet, June* 15, 1878, *p.* 882.

79.—ON CATHETERISM IN CASES OF STRICTURE ON PHYSIOLOGICAL PRINCIPLES.

By JOHN GAY, Esq., F.R.C.S., Senior Surgeon to the Great Northern Hospital.

Cases of stricture, I need hardly say, often come under the care of the surgeon, especially in hospital practice, in which, owing to the patient's neglect, a stricture barely permeable becomes almost suddenly impervious, and the surgeon is called upon to procure a passage of some kind for the urine in the teeth of every obstacle, normal and abnormal, that can waylay his efforts and render them difficult.. It is to the earlier period in this (the culminating) stage of such a case that the following remarks are designed to apply.

A man, aged twenty-eight, recently presented himself at the Great Northern Hospital during my visit. He had suffered from stricture for years; had had urethral discharge in abundance, and chronic balanitis as well. Latterly his urine had dribbled away, and, before reaching the hospital, this resource had failed him. Catheterism was attempted by skilled hands, but in vain; and as early relief was necessary, an operation was advised, but refused. On examination, he was found to have a hard, firm, and painful stricture about three inches from the orifice, for which I proceeded to use a catheter on the following principles.

1. As it is, the urethra is absolutely impervious to the passage of the catheter from a combination of causes—viz., the stricture growth engorged with mucus and blood, and rendered painful by futile catheterism; and certainly spasm. It is not, however, absolutely impassable.

2. The tightest part of stricture is that in front.

3. The unconditional use of a catheter would, in such a state of the parts, certainly intensify the difficulty by calling into play a new source of resistance, in the form of normal muscular antagonism, to its passage—a force that is ever on the alert to oppose the enforced passage of a foreign body through the urethra into the bladder.

4. This automatic force can be brought under complete control by an act of volition, and not only so, but be made to impart to the strictured canal the greatest amount of patency and passivity of which it is capable.

5. The means to this end consist in making the patient bring the sphincters or detrusors of the bladder and urethra into a state of absolute rest by voluntarily, but gradually, calling into powerful action their antagonists, the expulsors or accelerators, and using the catheter whilst the force thus elicited is kept in a state of strain.

6. This mode of palsying the detrusors has another advantage which anæsthesia does not possess, since it assists the surgeon by employing the urine as a dilator, and thus reduces the resistance of the stricture slit.

In the case before us the method thus indicated was carried out as follows. The patient was made to stand, supported by assistants, upright against a firm support, with outstretched legs—a position I always insist upon in catheterisation, if feasible,—and being prepared with a well-warmed and oiled silver catheter (No. 4, at a venture, in this case), he was called upon to make an effort to pass his water and to gradually increase it to the extent of his power, always under the impressed conviction that he will succeed. After straining thus for a few seconds, and being required to keep up the act until he had permission to relax it, the point of the instrument was gently insinuated into the urethra, and carried on to the stricture. By careful exploration I was soon satisfied that its point and the slight force I was using were in a line with the axis of the canal, and that the entrance of the stricture had been reached. This I *felt*, for I had contrived to slide the instrument along the floor of the passage to the furthest point I could reach in any part of the canal, and by the sense of a slight grip of its point which was given me on making a simple move of the instrument onwards, I was sure that the passage had been gained. The patient still keeping up the strain, with a very little more force the catheter passed through with the usual, not always assuring, jerk. It could not, however, be made to enter the bladder, for its course was interrupted by another stricture at the membranous part of the urethra. This I did not attempt to pass, being satisfied that if the instrument could be retained during the night, the remainder of the passage would be easily passed in the course of the morrow, for the catheter would now indirectly act as an expulsor, and therefore keep in check any renewal of action on the part of any counteracting power. The urine passed abundantly during the succeeding night, not *through* the catheter—for it contained some clotted blood, and if it had not, I should have prevented it by the use of a close-fitting stilette,—but around it; and on my visit the next day, the instrument was passed through with the help of the tip of my forefinger. A severe rigor followed the first effort, which was subdued by a glass of hot brandy-and-water and one scruple of quinine in the course of the next twenty-four hours.

The subsequent treatment has been daily catheterisation, using a larger catheter each day, and allowing it to remain a few hours on each occasion. On the seventh day a No. 8 was easily passed. I need not refer to the watchful care which is always needed in the after-management of such cases.

I have ventured to ask permission to publish this case, trusting that the principle advocated—viz., that of falling back upon physiological resources as a help in the treatment of severe cases of stricture—might meet with whatever attention it may be thought to deserve.

I may state that I insisted on this method of treating stricture in a paper published in June 1861; and that, although it called forth but little attention at the time, and I believe less since, I have not failed to employ it in every case requiring it, with invariably the like results. I have also demonstrated it in the course of hospital and private practice, so that it is not entirely without its witnesses.—*Lancet, Nov.* 16, 1878, *p.* 687.

80.—ON THE EFFECTS OF ALKALIES ON URINE.

By Dr. C. H. Ralfe, Physician to the Seaman's Hospital, and Teacher of Physiological Chemistry in the Medical School, St. George's Hospital.

Effect of Bicarbonate of Potash on the Acidity of Urine.—Twenty-eight years ago Dr. Bence Jones, in a paper read before the Royal Society, showed conclusively, from a series of observations, that large doses of sesquicarbonate of ammonia not only did not diminish the acidity of the urine, but actually increased it; as he found that the day when most carbonate of ammonia was taken the acidity was higher than it had been any previous day, and that the acidity of the urine was still very high on the day following its discontinuance. Four years later a similar observation was made by Dr. W. F. Beneke with regard to the effect of bicarbonate of soda. And, lastly, Professor Parkes, in his work on the Urine, published in 1860, instances a case of rheumatic fever in which the acidity of the urine on the day following the administration of bicarbonate of potash was considerably higher than on the day before it was taken.

It is strange that observations made by authorities so eminent and trustworthy, tending to controvert the generally received opinion as to the action of these alkaline bicarbonates as antacids, and establishing the contrary proposition that they increase rather than diminish the acidity of urine, should have attracted so little the attention of writers on therapeutics or urinary pathology in this country or abroad.

As the question is of considerable importance with regard to treatment, especially of those affections which are caused by the excessive formation of acid within the system, I have thought an account of some observations made by myself on the action of one of these carbonates—the bicarbonate of potash—would be of service, not merely in calling attention to an important

therapeutic fact, but also in determining the circumstances that induce this apparently anomalous action, and in endeavouring to explain the paradox of these alkaline salts increasing instead of diminishing the acid reaction of the urine.

The first series of observations were made to test the effect of bicarbonate of potash on the reaction of the urine when taken before meals. The quantity of the salt taken was two drachms in the twenty-four hours—namely, one drachm at 12 p.m. (one hour before dinner), and one drachm at 8 p.m. (one hour before supper). The physiological conditions of food, rest, and work were kept as equal as possible.

[Dr. Balfe gives a very interesting set of tables illustrating the effects of bicarbonate of potash, and ends by saying]

The effect of bicarbonate of potash, taken after food, on the acidity of the urine, is different from that when it is administered before meals. For when taken on an empty stomach we have seen that the acidity on the day of administration was only slightly depressed, whilst on the day following the acidity was considerably higher than it was the day before the salt was taken. But when it was administered during the process of digestion the acidity of the urine entirely disappeared, being on two occasions neutral, and on one alkaline, whilst on the succeeding days there was no marked increase in the acidity of the urine as compared with that of the days preceding the experiment. And the same difference is observable in the hourly variations of the urine, for when the bicarbonate was taken before meals the effect of the alkali passed off at the end of two hours, and the amount of acid passed in the succeeding three hours was nearly equal to what was passed on the day no medicine was taken; whilst when the salt was taken after meals the urine remained alkaline up to the end of four hours after the dose was taken, and no recovery of acidity was noticeable.

The result of these observations tends to establish the fact that the administration of an alkaline bicarbonate on an empty stomach increases the acidity of the system, whilst its administration after a meal diminishes it.

How is it that these alkaline bicarbonates have such opposite effects? Neither Beneke nor Parkes offered any satisfactory explanation of the phenomena recorded by them, the latter only giving the results of his observation as a "contribution towards a future inquiry." Dr. Bence Jones, however, came to the conclusion that the increase of the acidity of the urine in the case of sesquicarbonate of ammonia was caused by the ammonia, in its passage through the system, giving rise, by its

decomposition, to the formation of a certain quantity of nitric acid. The fact, however, that a similar increase is noticed when the bicarbonates of other alkalies are administered, shows that this explanation is not in their case the true one.

The explanation that has occurred to me as the most probable, and by which the variable effect of these salts on the acidity of the urine can best be accounted for, lies in their chemical constitution, the alkaline bicarbonates being acid salts. Now, the acid reaction of the urine is generally considered due to the composition that occurs between an acid or an acid salt and the neutral phosphate of sodium in the blood, acid sodium phosphate being formed, which passes out with the urine. Now, one of the chief acid salts in the blood is undoubtedly bicarbonate of potash or soda, an acid salt with an alkaline reaction. The decomposition which results between these two salts may be represented as follows:—

Acid carbonate.		Neutral phosphate.		Normal carbonate.		Acid phosphate.
NaH_2CO_2	+	Na_2HPO_4	=	Na_2HCO_3	+	NaH_2PO_4

It is, therefore, not surprising to find the administration of an acid salt, if it passes unaltered from the stomach into the blood, causing an increase in the acidity of the urine. And this is, indeed, what happens when a dose of bicarbonate of potash and soda is taken into the stomach before meals, for then, the mucous membrane under normal conditions being either neutral or alkaline, the bicarbonate is absorbed undecomposed into the blood, and causes that increase in the acidity of the urine which has been noted. On the other hand, when the salt is taken during digestion, the acid contents of the stomach decompose it, carbonic acid is liberated which escapes by the mouth, whilst the alkaline bases pass into the system and cause the urine to assume an alkaline reaction.

The therapeutic indications to be drawn from these observations may be thus summarised:—

1. In cases of acid dyspepsia arising from the excessive formation of acid within the system, as in lithæmia, the alkaline bicarbonates should not be administered before food, but after.

2. The administration of alkaline bicarbonates before meals is indicated in those cases where the free acid is formed in the stomach itself, the result of fermentative changes of undigested food or morbid mucus, when it is necessary to diminish the too high degree of acidity thus caused in order to permit digestion to be properly performed.—*Lancet, Nov.* 9, 1878, *p.* 651.

31.—ON THE USE OF THE ACTUAL CAUTERY IN SURGICAL OPERATIONS.

By HENRY LEE, Esq., Consulting Surgeon to St. George's Hospital.

Before the introduction of anæsthetics into surgical practice, the use of the actual cautery was justly regarded as a barbarous and cruel proceeding. The traditions of one age descend to the next, and the application of a hot iron to the body is still regarded by the public, and even by some members of our profession, as a thing not to be tolerated in scientific surgery. All objections to the use of the actual cautery vanish, however, so soon as it can be said that it can be used without giving pain. We may, therefore, now fairly consider its advantages as compared with other modes of arresting hemorrhage.

For the last twenty-five years I have been in the habit of sealing bleeding vessels with the cautery, and have of late used it occasionally in operations of all kinds. In one case amputation of the thigh was performed, a single ligature being placed upon the femoral artery. In the three following cases of amputation of the leg the actual cautery alone was used.

Case 1.—Alfred C., aged twelve, was admitted into St. George's Hospital on July 26th, with infantile paralysis. There was a partial displacement of the right tibia on the femur; the lower part of the skin of the right leg was generally inflamed, and of dusky brown colour; the skin of the foot was extensively ulcerated, and in many places a sloughing action had commenced. Amputation of the leg below the knee was performed on August 3rd. Each bleeding vessel was touched with the black-hot cautery. No ligature or other method was adopted to arrest the hemorrhage.

8th. No hemorrhage had occurred.

10th. There was some little sloughing at the edges of the wound.

21st. Patient allowed to lie outside his bed.

28th. The wound had almost closed, and a few days later the boy left the hospital, and returned to show himself some weeks subsequently with an excellent stump.

Case 2.—Susan H., said to be fifty-six years of age, but looking much older, was knocked down by a heavy van, which passed over her legs, and was admitted into St. George's Hospital on February 15th, 1876. She had a compound comminuted fracture of the left leg about the middle, with much laceration of the soft tissues, and also a simple fracture of both bones of the right leg. Amputation of the left leg was immediately proposed, but to this she obstinately refused to consent.

Feb. 16th. She had wandered slightly all night. In the

afternoon of this day the leg was amputated at its upper third. Each bleeding vessel was touched with the black-hot cautery, and no other means of arresting hemorrhage was used.

17th. Had passed a good night, with improved pulse; but she sank from collapse during the following morning.

At the autopsy, the vessels on the surface of the stump were found firmly closed by coagula, as may be seen in the preparation in the museum of St. George's Hospital.

Case 3.—Richard S., aged thirty-six, a labourer, was knocked down by an engine, which passed over his foot; and was admitted into St. George's Hospital on Jan. 14th, 1876. The foot was found to be completely crushed. There was also severe contusion and swelling of the face, so that he could not separate the eyelids. Soon after admission his left foot was amputated, and the vessels sealed by the actual cautery.

Jan. 15th. There was some oozing of bloody fluid, but not sufficient to necessitate a removal of the dressings.

During the night of the 17th the oozing continued, and the wound was opened, when there was found to be blood coming from several small vessels but from none of the large ones, which had been touched by the cautery. The application of iced carbolic lotion sufficed to stay the bleeding.

On Jan. 21st the wound was covered with healthy granulations, but the patient did not leave the hospital until the 1st March, his recovery being delayed during the separation of a portion of the cut surface of the tibia.

When the facial swelling had completely subsided, a large depression in the centre of the frontal bone was plainly visible, and it became evident that the outer table had been driven in at the time of the accident.

In the present age of improved science, with the appliances of antiseptic surgery, the mode of operating with the actual cautery is not advocated as the best. It cannot, for instance, be conveniently used in conjunction with the ether or carbolic acid spray. Where, however, the means of carrying out the antiseptic measures are not at hand, the actual cautery has some very decided advantages over other modes of arresting hemorrhage. As yet the great majority of operations throughout the kingdom are still performed under a variety of conditions which forbid the possibility of the antiseptic method being fully carried out.

The application of the cautery coagulates the albumen in the blood as well as in the tissues to which it is applied, and, if used at a proper heat, will cause the parts to adhere firmly to the iron as well as to each other. It also has the effect of coagulating the blood in the vessels, as is shown in the preparation from Case 2. A very superficial layer, if any, of the parts

to which the cautery is applied is killed, and any tissue so killed is charred, so as to become not readily liable to septic decomposition.

The actual cautery has the great advantage of closing the veins as well as the arteries, and thus removes one of the gravest sources of anxiety to the surgeon, inasmuch as the great danger in surgical operations arises from the absorption of septic agents through unclosed veins. The ligature of a vein, as is well known, is not unattended with risk, for the obvious reason that during the time that the ligature is separating a certain amount of suppuration necessarily takes place, and the products of that action may give rise to septic absorption. The actual cautery, therefore, possesses a great advantage over the ligature, as far as the veins are concerned. The danger of blood infection, though in a much less degree, obtains when an artery is tied. In a vein the current of blood is towards the centre of the circulation, and in an artery from it. When mischief arises from the cause mentioned, after an injury to an artery, the effects are usually seen in the peripheral circulation. When venous absorption takes place, the results are generally manifested in the central organs.

It is not improbable that gangrene of an extremity, after an injury to the main artery of a limb, may arise from the circulation of vitiated blood more often than from a deficiency of blood. Simple want of blood is indicated by paleness of the skin. In the majority of cases of gangrene the skin contains far more blood than usual, and is of a dark-brown colour.

The septic action commenced around the ligature on an artery may be communicated to the surrounding parts, and from these septic absorption may take place through the veins; this, in fact, is one of the most fruitful sources of pyæmia. After the late American war, a most distinguished and honoured physician in Scotland entertained some of the surgeons of the Southern army, and one day after dinner remarked to them that he understood that they were in the habit of poisoning the wounds of the Northern soldiers committed to their care. The surgeons became very indignant. The physician persisted, and at last the explanation came—viz., that whenever a ligature was applied to an artery the distal portion of the vessel, together with any of the included surrounding tissues, sloughed, and became a possible source of putrid infection. No such sloughing follows the judicious use of the actual cautery. The actual cautery may be applied sometimes where a ligature cannot—as, for instance, in wounds of the deep palmar arch; but, on the other hand, a tenaculum will sometimes reach a vessel to which the actual cautery could not be effectively applied. In one case, where a very large tumour had been removed from the back,

the vessels were all sealed by the actual cautery except one, which bled persistently, but the reason for this became immediately apparent: the projecting posterior edge of the scapula prevented the cautery coming fairly in contact with the bleeding vessel.

I have now used the actual cautery in a great variety of operations, and during the last twenty-five years in a large number of cases of hemorrhoidal tumours, but as yet have had no case of purulent absorption following its use.

Within a week of the time I write, I assisted Mr. Hyatt, of Shepton Mallet, to remove a cancerous growth from the inner canthus of the eye. The bleeding was free and general, but on the raw surface being touched with the cautery all bleeding immediately ceased, and on the next day the patient expressed himself as feeling more comfortable than he had done for a long time previously. I prefer the common iron cautery to the benzoline "thermo-cautery" knife, when no dissection has to be made. The oxide of iron appears to exercise a direct influence in producing coagulation of the blood. Many years ago, when engaged in some physiological investigations, I received some blood from a horse that was being bled into an old iron vessel, coated on its inside with the red oxide of iron, and was surprised to find that the whole mass of blood became coagulated immediately.

Another, though minor, advantage of the cautery is that it can be applied much more quickly than the ligature; and this is by no means unimportant when many vessels have to be dealt with. Practically, it is difficult not to use the cautery too hot, as if it kill the parts to which it is applied the desired result is not produced. A good test as to the cautery being at the required heat may be obtained by dabbing the iron on a moderately wet sponge until only a little steam is generated. —*Lancet, Nov.* 9, 1878, *p.* 653.

82.—ON THE USE OF ETHYLATES OF SODIUM AND POTASSIUM AS CAUSTICS.

By Dr. BENJAMIN W. RICHARDSON, F.R.S., &c.

The great interest displayed at the last meeting of the Medical Society of London on the subject of the ethylates of sodium and potassium, which I introduced into medicine in the year 1870, leads me to think that a brief description of these substances and of their medicinal application may be of interest to the wider circle of medical practitioners who are readers of the Lancet.

The ethylates first came into my hands for study when I was conducting a series of experimental inquiries on the action of

the different alcohols. They are sometimes called alcohols because in them the atom of hydrogen which in alcohol is, with its radical, combined with oxygen, is replaced by an atom of sodium or potassium.

The change of ethylic alcohol into sodium alcohol transforms it from an irritant to a caustic. Laid on dry parts of the body the sodium alcohol is comparatively inert, creating no more change than the redness and tingling caused by common alcohol; but so soon as the part to which the substance is applied gives up a little water, the transformation I have described above occurs; caustic soda is produced in contact with the skin as water is eliminated by the skin, and there proceeds a gradual destruction of tissue, which may be so moderated as hardly to be perceptible, or may be so intensified as to act almost like a cutting instrument.

Potassium Alcohol, or *Potassium Ethylate*, is made in a similar manner as sodium ethylate—viz., by bringing pure potassium into contact with absolute alcohol. The action of the potassium is much more energetic than that of sodium. I prefer to immerse the potassium under the alcohol in a small glass bell, from which there is a tube to allow of the escape of the liberated hydrogen. When saturation is complete, a thick and almost colourless fluid is formed, from which the ethylate may be obtained in in a solid crystalline state. Exposed to water, the potassium ethylate is transformed, as is the sodium ethylate, into ethylic alcohol and potassium hydrate. The action of this compound on animal tissues living and dead, is the same as that of the sodium compound, but is more energetic.

Practical Uses of Sodium and Potassium Alcohols.—I do not as yet see the means of applying readily these two active alcohols for internal administration, but I can predict for them a very extensive application for external purposes. They are most potent caustics. In some cases they may be employed to destroy rapidly such morbid growths as are not favourable for excision by the knife. In many cases of cancer they will prove invaluable, and will, I believe, exert a direct local curative influence. Injected into morbid growths, they would so quickly destroy them that the action might have to be conducted while the body was under the influence of an anæsthetic.

In being applied direct to the sensitive unbroken skin, I find that their destructive action is less painful than would be expected. I have made with both compounds a superficial eschar on my arm with no more pain than a slight tingling warmth. What is more, when pain is felt, it may be checked quickly by dropping upon the part a drop of chloroform, which decomposes the alcohol, converting it into a chloride salt, and an ether—triethylic—which is inert locally. Again, I find that

these alcohols dissolve some of the vegetable alkaloids. Thus opium may be dissolved in them, and a solution of opium in caustic alcohol is made directly by mere addition of the narcotic to the caustic spirit. Practical men will see the advantages of combinations of these alcohols with narcotics. The practice opens the way to one of the greatest needs of medicine—a sure, rapid, and painless caustic.

The caustic alcohols may be used in combination with local anæsthesia from cold. A part rendered quite dead to pain, by freezing with ether spray, could be directly destroyed by the action of caustic alcohol—a practice very important in the treatment of poisoned wounds, such as the wound from the bite of a snake or a rabid dog. It is by no means improbable that some cystic tumours may be cured by the simple subcutaneous injection of a little of these fluids after destruction of sensibility by cold.

Potassium and sodium alcohol, added to the volatile hydride of amyl, dissolve in the hydride and produce a caustic solution. When this solution is applied to the skin, the evaporation of the hydride takes place, and a layer of the caustic substance is left behind. This application would prove very useful to the surgeon in many cases of disease.

The action of the ethylates on the blood is extremely rapid and marked. The red corpuscles are brought by it into solution, and there forms (quickly in some cases) an almost instant crystallisation of blood; the crystals are acicular, and spread out in arborescent filaments. The arborescent appearance is identical with the crystallisation of the ethylates themselves, but the smaller radiant crystals are due, I believe, to the crystallisation of the crystalloidal matter of the blood-cells. They are singularly like the crystalline forms which have been described, since the time of Dr. Richard Mead, as occurring in the blood after infection by the poison of the viper. One other peculiarity in the action of the ethylates on blood is worthy of notice: while they seem to attack and dissolve the red corpuscles vigorously, they act with comparative slowness on the white corpuscles, so that we may see a white corpuscle floating uninjured in a sea of red colouring fluid previous to crystallisation, and even adhering to the crystalline points after crystallisation.

The ethylates possess also powerful antiseptic properties, so that even nervous matter, which of all animal substance is most prone to decomposition, can be long kept in good preservation in the presence of them.

This was my report in the year 1876 on these ethylates, and since then I have used them often in practice with much success. The ethylate of sodium is the most manageable. It is very

easily made, and its caustic property can be changed in different specimens, to suit different cases. The solution I use is one of half saturation, and I keep the solution in a bottle having a glass rod descending from the stopper, the end of the rod being somewhat pointed. With this rod I lift the solution from the bottle and apply it from the point of the rod. At one time I used a glass brush for the purpose, but I found the brush objectionable, the small fibres of glass being disposed to break too easily.

The first idea that occurred to me in respect to the application of the ethylates was founded on their action physically. It seemed to me reasonable to suppose that if the ethylate were applied to a moist external growth there would be produced two changes: a caustic action would be set up, and at the same time the alcohol would cause instant coagulation of the fluids of the part, so that the destruction of structure would be purely local and concentrated. In addition to this there would be the advantage of the antiseptic action, which is so marked. Three forms of disease were, I thought, amongst the best for its use—cutaneous nævus, lupus, and malignant ulcer.

Treatment of Nœvus with Sodium Ethylate.—In 1870 I treated with the ethylate a case of nævus on the neck of a child two years old. Not more than six applications of the fluid were made when the nævus was entirely removed, and a sound surface was left. The nævus in this instance was so small I did not consider the trial of sufficient value; but soon afterwards my friend, Mr. Gay, was good enough to show me a case of nævus of the scalp in a child under his care at the Great Northern Hospital. The nævus was of the full size of a half-crown, and extremely prominent. It had been treated on various plans—by tying, by nitric acid, and other methods,—but without success. Mr. Gay having consented to my request that the ethylate should be applied in this case, I sent a specimen of the remedy to the hospital for use there. Instead of this the patient was sent to my house that I might apply the caustic myself. I commenced by covering the nævus lightly with the solution. The application gave very little pain, but soon a dark surface showed that the caustic had taken effect. Three days afterwards a firm hard encrustation had formed where the caustic had been applied, which encrustation I did not then remove. A few days later, the hard crust being loose, I gently raised it away to find the nævus greatly reduced in size. The ethylate was again applied to the surface of the nævus, and the same process was continued until the nævus was entirely removed, and a natural surface was left. The case was under my treatment nine weeks and three days. Some time afterwards the child was brought to me so completely well that it was not easy to discover where the nævus had been.

Dr. Brunton's cases, which he so ably reported at the Medical Society, are similar in kind, and his results are equally good. I referred at the meeting to some other cases in which I had used the ethylate. These I will notice at length on some future occasion. It is better now for me to give one or two practical hints for those who wish to use the ethylates.

Practical Notes.—In making the ethylate of sodium, which is the most manageable, it is best not to make much at a time. Put half a fluid ounce of rectified alcohol (sp. gr. 0·975) into a two-ounce test-tube, set the test-tube up in a bath of cold water, and add, in small pieces at a time, some cuttings of pure metallic sodium. A gas, hydrogen, will at once escape. Add the sodium until the gas ceases to escape, then warm the water in the bath to 100° F., and add a little more sodium. When the gas again ceases to escape, stop the putting in of more sodium; or, if crystallisation takes place, then stop. Afterwards cool down to 50° F., and add half a fluid ounce more of alcohol. This will give a good working solution, which can be made more active by adding sodium, or less active by adding alcohol.

Put the solution in a glass stoppered bottle, and have the bottle always well closed.

Keep the bottle always in a cold place. Once, a bottle of the ethylate, left during summer time exposed to the sun in my laboratory, exploded, so that the bottle was broken and the contents spilled.

Always apply the ethylate with a glass rod.

The ethylate solution must not be mixed with other fluids than alcohol. Mixed with chloroform, in quantity, a violent action is set up, and the ethylate is decomposed into chloride of sodium and an ether—triethylic.

The ethylate is not so manageable for subcutaneous injection as for application to the surface by the glass rod. It specially deserves trial in lupus, in malignant ulcer, and in vascular cutaneous growths and excrescences.

The addition of an alcoholic solution of opium lessens the pain of an application.—*Lancet, Nov.* 9, 1878, *p.* 654.

83.—TREATMENT OF LEPROSY.—CHAULMUGRA OIL.

By Dr. DAVID YOUNG, Florence.

Chaulmúgra Oil is a fixed oil obtained from the seeds of the Gynocardia Odorata, one of the Bixineæ, found in the forests of Eastern India. From time immemorial it has been held in great esteem by the native races of India as a potent remedy in the treatment of leprosy, and interesting notices of the oil in its relation to this disease are found in several of the Hindu

works on Materia Medica. About eight years ago public attention was directed to the subject in Western India, in consequence of several cases of reported cure under the care of Dr. Bhau Daji, a graduate of the G.M.C. Bombay. The native press gave prominence to these reports, and the result was that a great many lepers came—some from very long distances—to Bombay for the purpose of consulting him. However, the fees he demanded were exorbitant, and the discovery being soon made that his treatment was mainly a revival of that practised by the Hakims centuries before, so that his fame was short-lived, though longer than it deserved. A number of the lepers who had been attracted to Bombay by Dr. Bhau Daji's fame applied at the Mission hospital for relief, and in the course of eighteen months between fifty and sixty cases were treated with the chaulmúgra oil. During this period my attention was directed to a leguminous plant, the Psoralea Corylifolia, which abounds in the Konkan and Deccan, and was said to be much used by lepers as an external application, in combination with oil. While investigating this I discovered that the seeds of the plant were well known to Hindu ladies, and were much used as a remedy in *loss of hair*.

The following notes include cases treated simply with chaulmúgra, and others treated with chaulmúgra and psoralea.

The forms of the disease noted were:—Macular Leprosy, 4. Anæsthetic Leprosy, 23. Tubercular Leprosy, 15. Mixed cases, 11. The patients were all adults, and the proportion of males to females was about 3 to 1.

[Dr. Young gives six cases, and makes the following comments]

The *first* and *second* are pretty well marked examples of macular leprosy, the *third* is a good specimen of the anæsthetic form, developing out of the macular variety, while the *fourth* began with and maintained throughout the anæsthetic type. The *fifth* is also an example of anæsthetic leprosy, complicated by ulcerous sores, the remains of unhealed bullæ and the conical ulcer so frequently seen in this form of the disease, and the *last* is a case of tubercular leprosy, presenting well-marked anæsthetic symptoms.

Treatment.—The two drugs employed were the chaulmúgra oil and seeds, and a preparation of the seeds of psoralea corylifolia. The chaulmúgra oil usually found in the bazaars being very impure, supplies of the purest oil were obtained from Calcutta. A tincture of psoralea was prepared by macerating ten drachms of the seeds in a pint of rectified spirit, and a liniment by adding a drachm of the tincture to an ounce of chaulmúgra oil.

First Case.—Treatment commenced April 17th. Twenty

drops of the chaulmúgra oil were given three times a day, and the affected parts were rubbed with the oil morning and evening.

On April 19th, patient complained of great nausea. The dose of the oil was reduced to ten minims, but with no diminution of the sickness.

On April 21st he began to take five drops three times a day, and continued for a month without any feeling of sickness. The dose was gradually increased until at the end of three months he was taking fifteen drops three times daily.

After four months' steady treatment a marked improvement began to show itself. The blotches gradually lost their raised margins and the "centres" lost their pale dry appearance and assumed a healthy look. There was more "feeling," as the patient expressed it, in the large patch on the left side of the face, and in ten months from the beginning of the treatment the face was almost natural. The large blotch was nearly gone, and except in one little spot on the ala of the nose sensation was quite restored. Patient gained 4 lbs. in flesh. On the restored parts a few sickly-looking hairs had appeared.

Second.—Treatment begun April 24th. Twenty drops of chaulmúgra with ten of tincture of psoralea were given three times a day. On April 28th patient complained of sickness. He had vomited the last two or three doses of the medicine. Six drops of the oil with six of the tincture were now given, and this the patient bore well, and continued for about six weeks when it was increased to ten drops of each, and so gradually on till the dose was twenty drops. From the commencement he used the liniment of the oil and psoralea. This patient improved more rapidly than the previous one. In three months from the beginning of the treatment the spots had undergone a considerable change, and the eyebrows were almost natural, while the hairs on the other parts of the body were numerous and strong. In October—that is six months from his admission—he returned to his country, having meanwhile gained seven pounds in flesh with the spots almost completely gone. Sensation quite restored in the patch on the leg and helix of ear.

Third.—Began to take five minims of the chaulmúgra oil three times a day on 4th of May, and at same time to use the liniment of the oil and psoralea. Continued for two months without any nausea. The dose was slowly increased to fifteen drops of each. Improvement was very tardy, first showing itself by the patient being able to grasp objects with less pain than formerly. The coffee-coloured spots gradually paled and several of them entirely disappeared. The numbness in the *centres* of the spots diminished, but never completely went, and

no apparent change took place in the shiny spots, save a slight thinning of their raised edges. No perceptible change took place in the shrivelled contracted fingers of the left hand. The lobes of the ears became less painful, but lost none of their thickness. The eyebrows were well covered with strong dark hairs and in all the bare spots there were vigorous growths of healthy hair. No note of weight.

Fourth.—RUN. commenced taking ten drops of the oil on 21st May. After four days he complained of pain in stomach and sickness. The dose was reduced to five drops and the compound liniment was used night and morning. In August the dose was increased to twenty drops, but there was very little improvement. During the first six months he had no return of the attack of pain, but twice had sharp fever which gave way to quinine. The hairs on the eyebrows appeared and also on other parts of the body, but not so healthy and strong as in the other cases. There was no change in the whitish spots, but the dark spots on the thighs became more natural in colour, and in these the hairs were healthy and numerous. Gained five pounds.

Fifth.—Began with five drops of the chaulmúgra which were rapidly increased to twenty. Patient gained flesh, but seemed to derive little or no benefit as regards the leprosy. The superficial sores healed after five months' treatment and the use of the compound liniment, but the conical sores remained as they were. The oil was continued for a whole year, and during three months the deep sores in the soles of the feet were dressed with carbolic acid and chaulmúgra (1 in 30). The sores gradually healed and remained so for two years, after which the patient was lost sight of. Beyond this no appreciable improvement could be seen; save a few hairs on the eyebrows no new growth of hair was observable. Gained eleven pounds.

Sixth.—The above treatment was persevered in for nearly nine months, and with the exception of a slight lessening of the tenderness in the tubercular spots there was no apparent benefit whatever.

Observations.—(*a*) In the macular and in the early stage of the anæsthetic forms of leprosy the chaulmúgra appears to be of decided value.

(*b*) The oil should be given at the outset in small doses and gradually increased, as otherwise it is apt to cause nausea.

(*c*) The good results were seen earlier in those cases in which the powdered seeds were given instead of the oil.

(*d*) A liberal milk diet seemed to be a valuable auxiliary.

(*e*) The psoralea given internally, in combination with the oil, appeared to be of no value, but when applied externally in the form of a liniment was of undoubted service, especially in stimulating the hair bulbs.

(*f*) Several of the cases treated were complicated with bronchial affections, which were quite relieved during the treatment. This, taken in connection with the fact that all the patients gained flesh, may point to the probable usefulness of the oil in affections of the chest.

(*g*) The oil has a deserved reputation in cases of itch and parasitic pediculi, and forms a valuable addition to the ordinary sulphur ointment.—*Practitioner, Nov.* 1878, *p.* 321.

84.—RODENT ULCER (FLAT EPITHELIOMA) OF SKIN TREATED BY ERASION WITH A SHARP SPOON.

By Dr. BALMANNO SQUIRE, Surgeon to the British Hospital for Diseases of the Skin.

This considerable improvement on the surgery of excision as applied to certain progressive ulcerating lesions of the skin was originated by Volkmann of Halle. It is nothing more than an extension of the use of the gouge (as used in removing carious bone) to a still wider application in the treatment of more or less similar diseases of the skin. Or from another point of view it may be considered as a hint adopted from the dentist who arrests the process of caries in a tooth by scraping out the carious part, and then inasmuch as a tooth is incapable of cicatrisation, constructs an artificial cicatrix by plugging the tooth-ulcer with an incorrodible metal.

The process has lately been largely adopted in uterine surgery in the treatment of various lesions of the cervix by scraping away the *materies morbi* by means of the so-called *curette*, and it has also been employed for the treatment of the commonly occurring and obstinate glandular nodules of the pharynx, commonly known as the "granular throat."

In Germany, where the method first received its application to the treatment of disease of the skin, its use for this purpose has been received with considerable favour; and it has met with warm support in Austria and in France and in America; but in this country certainly it has not met with approval. However, I am disposed to think that only one obstacle exists to its popularity with the profession in this country, and that the obstacle in question is a remediable one. It is this—namely, that the method has not been tried. It is with a view to its obtaining a trial that I now venture to report these cases.

Case 1.—Dr. X., practising in London, aged seventy-six, had been affected with rodent ulcer for eleven months. The ulcer was situated on the right temple just in front of the pinna of the ear. The ulcer was still of small dimensions. It measured half an inch across, including in this measurement its characteristic beaded edge. I may save the need of a detailed descrip-

tion of it, and more briefly verify my diagnosis of it as a veritable rodent ulcer by stating that having this report of the case in view, I availed myself of the obliging good nature of my friend Mr. Jonathan Hutchinson, who kindly examined our *confrère* for me, and furnished me with his independent view of the diagnosis. He pronounced it to be a rodent ulcer beyond any manner of doubt. On May 23, 1878, I operated (chilling the skin first by the ether spray), by means of one of my own sharp spoons carefully scraping away all the friable tissue, including the raised beaded margin. The process was effected without pain, and even without sensation of any kind except the sensation of cold occasioned by the ether spray. No blood, not even a drop, was lost. Fourteen days after the operation the wound was completely healed, and the patient consequently cured. No re-appearance of the disease has yet occurred.

Case 2.—Charles P., a man aged fifty-five, was admitted an in-patient of the British Hospital for Diseases of the Skin on March 20, 1878. He had been affected with rodent ulcer for six years. The ulcer was situated on the right side of the upper half of his nose, and measured half an inch vertically by a quarter of an inch across. The ulcer presented the characteristic floor and margin of a rodent ulcer; but in this, as in the previous case, Mr. Hutchinson most kindly sent me his independent diagnosis of the lesion, and called it a case beyond question of rodent ulcer. On March 21, I operated on the man under chloroform, scraping away the friable floor and margin with one of my sharp spoons. After the slight bleeding had (under digital pressure) quite ceased, the ulcer was pencilled freely with solid nitrate of silver. On May 24 the ulcer had completely healed up and the patient was dismissed cured. I have seen the patient occasionally from time to time since the complete cicatrisation of his ulcer, and no sign, not even the minutest perceptible trace of disease has evinced itself.

Case 3.—Philip J., aged seventy-eight, admitted in-patient of the British Hospital for Diseases of the Skin on March 7, 1878. Had been affected with rodent ulcer for four months. The ulcer (situated on the lower part of the right side of the face—namely, over the lower jaw) measures an inch and a half long by an inch across. The hinder margin of the ulcer is about an inch from the pinna of the ear, and the lower margin corresponds with the lower edge of the jaw bone. The ulcer presented a pale yellow superficially lobulated floor, irregularly depressed below the level of the raised margin of the ulcer. The sinuous raised beading or moulding which surrounds the ulcer is of almost cartilaginous hardness to the touch, of a faint pink-white colour, and measures across its breadth on the average one-eighth of an inch. At some parts

of its course this raised moulding is slightly lobulated or moniliform. I trust I have here said enough to establish the diagnosis. Indeed, the ulcer was in all respects a very characteristic rodent ulcer. There was no perceptible enlargement of the adjacent lymphatic glands. On March 7, 1878, I operated under chloroform by scraping away the floor and raised rim of the ulcer with one of my spoons, just as in the other cases. The floor of the ulcer came away quite readily, being quite rotten (as much so even as lupus-tissue), and the raised margin, notwithstandng its firmness to the finger touch, yielded to the spoon with equal readiness. April 13: The wound measures now only three-eighths by one-eighth of an inch, and is nearly closed. 16th: Wound now reduced to a pin-hole. From this date until June 1, one or two small discoloured spots in the skin, which remained or became developed in the otherwise perfectly healthy cicatrix, were dealt with at various times by scraping them out by means of the small spoon. The patient readily tolerated these minor interferences without anæsthesia either by the spray or by chloroform. In all about half a dozen of these supplementary erasions have been performed, and a few days ago on presenting himself for inspection, it was observed that a small speck of about the same size as the others—namely, of about the size of a millet-seed—has again declared itself.

Commentary.—The process of erasion originally advocated by Volkmann for the treatment of lupus has since been extended to the treatment of some other lesions of the skin, and amongst them to epithelioma in general, by Hebra junior, of Vienna, and F. J. Pick of Prague, and to rodent ulcer by Auspitz of Vienna. However, Auspitz supplements his scraping by the use of arsenical caustic. The instruments in use at Vienna for the treatment by erasion present to my mind two disadvantages: they are too large and too shallow; so that the former disqualification renders them deficient in precision, since it is necessary that every portion of the diseased structure should be effectually scraped away, otherwise it will at once begin to grow again. Now, these large spoons will not serve for a thorough scraping out of small corners. On the other hand, their shallowness prevents their edges effectually catching the morbid fragments so as to bring them readily away, unless the spoon be held perpendicular to the skin—and this position of it is of considerable practical inconvenience in exercising the required manipulation with them.

I have accordingly devised much smaller and deeper spoons of an oval form which I find are practically not only more convenient, but more efficient also.

In either case the spoons are made of steel and have sharp

edges. The Viennese spoons may be obtained of either Leiter or Reiner, surgical instrument makers, of Vienna. Those I have had constructed are to be had of Messrs. Weiss.

The floor of the rodent ulcer is extremely friable and rotten in texture, and so equally is the thin, and to the touch seemingly hard, margin of the ulcer. The friable texture comes away like soft putty under the touch of the spoon, while the tough subcutaneous fibrous tissue, as well as the equally fibrous tissue of the surrounding sound skin, is quite impervious to the touch of the spoon. In this way, by exercising only moderately firm pressure with the spoon, the sense of touch greatly supplements the eye in differentiating the diseased from the sound structure.

Pick (of Prague) recommends going over the ulcer with the spoon very carefully again about two days after the first operation, in order that any deficiencies of the first operation may be felt out and made good at the second procedure; but I myself, while fully approving of his precaution, prefer not to wait so long. I find that by exercising firm pressure on the scraped out ulcer with the finger-tips immediately after the first scraping, the bleeding becomes after about five minutes completely arrested, and that thereupon the second or supplementary scraping may at once be performed quite as conveniently and as efficiently as on the second day.

As to the after-treatnent, it is merely that of any simple and healthy ulcer—that is to say, it may be almost anything, or even nothing, according to the fancy of the operator. In all of the three cases above quoted I confined myself to merely washing the ulcers gently with soap and water once or twice a day, and keeping them greased with a film of simple ointment without other covering of any kind. It will be noticed that in two of the cases—namely, in those in which the ulcer was small, and its progress slow—the cure has been at least for the present quite complete, and that in the larger and more active ulcer presented by the third case the ulcer was immediately reduced to very insignificant proportions, although some supplementary interference was requisite, and perhaps a little more still may be needed to complete the cure.

Chloroform was used in two of the cases, because I had at the time not yet found, as I did when I had got to the third case, that chloroform is needless. I had thought it essential because I had found that if the surface were rendered insensible by the alternative of chloroform—namely, by the ether spray—the frozen condition of the skin interfered materially with the differentiating sense of touch, since all of the structures, sound as well as diseased, were rendered, when frozen, equally hard to the touch of the spoon; but I found out quite

in time for my third case (the case of the doctor), that if after freezing the ulcer and its immediate vicinity the skin be allowed to thaw, and if directly on its thawing the process of scraping be expeditiously performed, the operation may be effectually concluded without being in the least degree felt; indeed the doctor assured me that he felt nothing whatever.

I have now to contrast the merits of this procedure with the other alternative methods of arresting the progress of rodent ulcer, and they are but two, namely, excision and deep cauterisation. Now in the case of a disease which is so prone to attack by preference the face, and the chief and in most cases the sole danger of which is the ravages it commits in destroying the features so disastrously as it is wont eventually to do, it therefore becomes a great point in arresting its spread not to add to its ravages by so doing. Now, excision and deep cauterisation, although unquestionably efficient, present both of them this grave disadvantage. Then again, the process of erasion is a much more trival procedure, and dispenses with the need of chloroform, and so is more willingly undergone by a patient who, as yet, has only a trivial-looking and small-sized ulcer to urge him to submit to any formidable-seeming procedure. Then as to deep cauterisation, even chloroform administered at the time of application is unavailing to avert the subsequent and long-continued severe pain which the patient must inevitably endure; whereas there is no subsequent pain when erasion is performed.

Then as to the eventual result; will erasion insure against a future relapse? I am not yet in a position to say, although I should scarcely think it would; but then no more does deep cauterisation or excision.

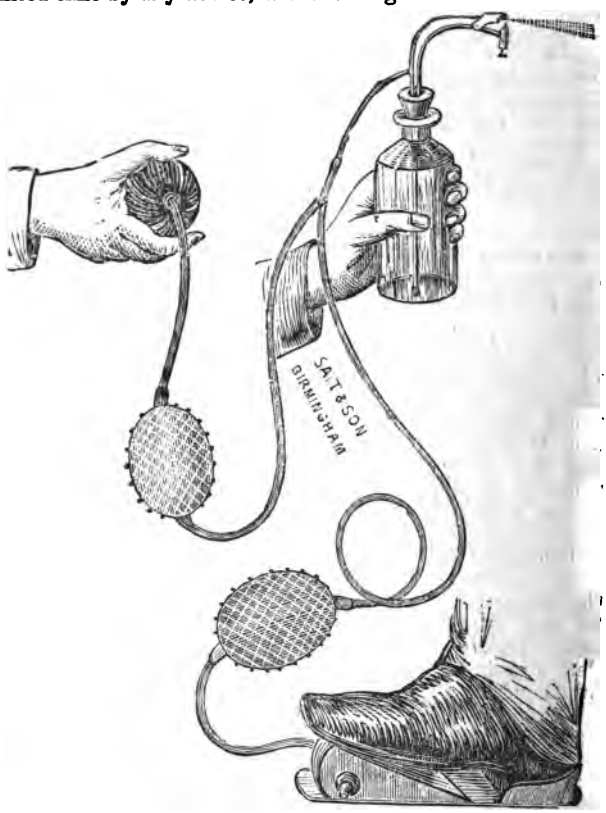
It is therefore probable that, whatever course may be pursued with reference to rodent ulcer, it will nevertheless turn out, if the further progress of the case be watched, that no course is capable of preventing the possibility, and perhaps not even the probability, of a future relapse; we must at the most content ourselves with healing the ulcer completely and effectually for the time. Now, this end, as I venture to affirm, is effected more conveniently, more economically as regards needless destruction of sound tissue, and more painlessly—in short, in a better and more artistic manner—by erasion than either by cauterisation or excision. If I have proved so much, or, rather, if a more extended trial of the operation in more experienced hands shall be found to prove as much—and I feel convinced that it will—then I venture to say that erasion will become the orthodox treatment of rodent ulcer, to the exclusion of the more formidable methods of excision or of deep cauterisation.—*Medical Times and Gazette, Nov.* 2, 1878, *p.* 512.

85.—NEW LISTER'S ANTISEPTIC SPRAY, IN WHICH THE HAND AND FOOT BELLOWS ARE COMBINED.

Mr. W. A. Frost, F.R.C.S., gives the following account of this apparatus, devised and manufactured by Salt & Son, Birmingham, at his suggestion, of which an engraving is appended:

"I think the carbolic spray apparatus, shown in the accompanying woodcut, will prove of service in practice.

"I have found that with the spray producers ordinarily in use, a very few minutes suffices to tire the worker; but by using the hand and foot alternately a spray can be kept up for an unlimited time by any novice, without fatigue.



"In long operations a larger bottle can be used, the tube within it being lengthened by a piece of india-rubber tubing. This apparatus has now been in use for several months at the North Staffordshire Infirmary, and has given great satisfaction —it has been used for most of the minor operations, for all the antiseptic dressings, and for an ovariotomy which lasted over an hour and a half, besides some other major operations.

"For major operations in hospital practice the steam spray producer possesses, of course, obvious advantages; but for private practice this instrument possesses qualifications which will render it of great service. Being light and occupying a small space, it can readily be carried, it can be worked by any any one; its construction is so simple that any defect in the working can be at once seen and remedied, and the cost is moderate.

"The instrument has been made by Messrs. Salt & Son, of Birmingham, and their name is sufficient guarantee of the excellence of the workmanship. It will be seen by an error of the draughtsman, the foot and treadle have been reversed, so that the former does not appear to belong to the same individual as the hands."

86.—CLINICAL THERMOMETERS WITH MAGNIFYING LENS.

The glass cylinder of which these thermometers are constructed is so contrived that the part overlying the index becomes a powerful lens, which magnifies the mercurial column so as to render it distinctly visible even to defective sight, without destroying the sensitiveness dependent on the employment of a tube with very fine calibre. The idea is patented, and specimens were exhibited at the meeting of the British Medical Association at Bath, in August, by Messrs. Salt & Son of Birmingham, who have arranged with the patentee to employ the magnifying lens to the various thermometers invented by them. They are made in three forms—with a reversible slip sheath, with a pencil-case movement, and with Fahrenheit and Centigrade scales combined. The two last are thus described by the inventors.

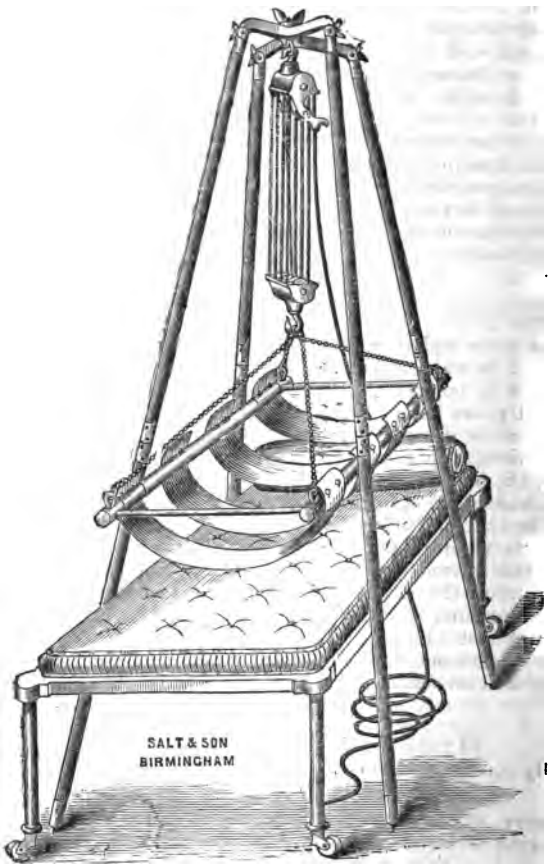
The thermometer is fixed into an *aluminium* case, and is *propelled* and *retracted* in a manner somewhat similar to the American pencils—viz., by rotating the small end to the right or left respectively. In addition to being portable when sheathed and long when protruded, it is the only instrument in which *case and thermometer are inseparable;* the shape of the case (fluted) prevents it from rolling off the table; and as, when retracted, the thermometer is wholly within the case, it is protected from being broken whilst the index is being replaced.

The instrument is neat in appearance, and convenient for the waistcoat pocket or the ordinary dressing-case.

87.—NEW SURGICAL HAMMOCK OR INVALID LIFT.

By SAMPSON GAMGEE, Esq., F.R.S.E. (Designed and manufactured by Salt & Son, Birmingham.)

The fracture-swing illustrates the apparent paradox, that the possibility of easy motion is necessary for the enjoyment of



perfect rest. What is true of a limb is equally true of the whole body. In cases of injury or disease of the spine or pelvis, it is very difficult to raise or move the body without displacement or pain, and even worse results. On my suggestion, Messrs. Salt & Son have constructed an apparatus in which the pulley blocks already described play a very important part. By these means a hammock and lifting apparatus can be put together over the bed, so that one person can raise the heaviest patient, retain him in an easy swing for any length of time, and lower him on the bed whenever required without jerk or effort.

Under the patient, and next the night-dress, are to be placed some soft wide pieces of webbing, of sufficient length to allow scope for the movements of the body within the rods lying on each side of it. These rods are connected with the ends of the pieces of webbing so as to secure strength, and be easily removable. When it is necessary to lift the patient, a stout four-legged derrick is adjusted with two of its legs on each side of the bed, having a powerful pulley, on the model described, suspended by a hook to the point where the four legs converge and unite. The inferior block of the pulley is armed with a hook, which grasps the chains connected with the rods lying on each side of the patient. To prevent these rods approximating too closely and squeezing the body uncomfortably, transverse bars are fitted into the sockets at their extremities.

The body is thus hung in a hammock, which can be raised or lowered by one attendant very gradually, and with the certainty that when the cord is drawn to one side the pulley will catch instantly and immovably; and run smoothly again, as soon as the cord is drawn straight down in the middle line. Thus suspended, a patient can rest comfortably for any length of time, the bed can be made, and any surgical dressings applied with ease and comfort.

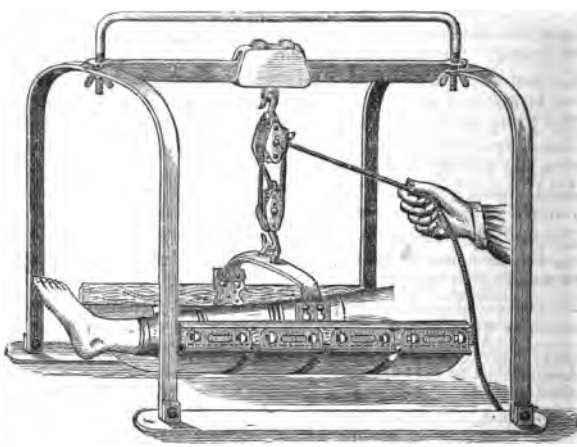
When the patient is lowered, the derrick, frame, pulleys and subsidiary adjuncts are removed, while the webbing, straps, and longitudinal rods may be left in readiness for future use. Thus in hospitals the elevating apparatus may be made available for many patients, the separate webbing straps alone being required for each bed. For greater convenience the poles unscrew in their centres, and can be separated by means of thumb screws from the cruciform connexion which sustains the pulleys.—*Lancet.*

88.—AN IMPROVED FRACTURE SWING.

By Sampson Gamgee, Esq., F.R.S.E. (Designed and manufactured by Salt & Son, Birmingham.)

Every surgeon knows how much relief can be given by swinging an injured limb; but with the ordinary apparatus little more is done than to raise the limb, and reduce engorgement by

facilitating the return circulation. In a case of fracture, movements of the limb or body are felt, with all their evil results, at the seat of injury, in accordance with the well-known principle that the resistance of an object is only equal to that of its weakest point. When the limb is slung, if the medium of suspension be, as it should be, very easily movable, it, and not the fracture, becomes the weakest point; and any disturbing force is expended in the suspending medium, while the limb remains at perfect rest. It follows that to ensure this great desideratum—the communication of the least possible impulse to the point of fracture—a swing should be as movable as possible, so as to exhaust in its undulations the motive power communicated to the limb, and thereby render impossible a jerk in any part of its length. This object is very imperfectly attained by the swing in general use; the chain is stiff, and to lengthen or shorten it with the suspending hook involves discomfort and often pain. These evils are all removed by the substitution of a fine rope for the chain, and by using the pulley-blocks as adapted by Messrs. Salt & Son. By means of this appliance, the *modus*



operandi of which is clearly illustrated in the drawing, the fractured limb may be raised or lowered to any desired extent (within the limit of the height of the frame), without the slightest possibility of any jerk or strain, and a very desirable relief afforded to the patient during the enforced period of inaction. The pulley, being self-supporting, requires no fastening; it cannot become entangled, and admits of an unskilled attendant raising or lowering the limb for the patient's comfort, without the possibility of displacing the injured parts.—*Lancet.*

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